
HOMINES DEOS ACCEDUNT HOMINIBUS DANDO SALUTEM
N.E.A.M.

TRANSACTIONS

OF THE

NEW YORK

ACADEMY OF MEDICINE.

INSTITUTED 1847.

SECOND SERIES. VOLUME ONE.

UNA FIDES, ALTARE COMMUNE.

NEW YORK:
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1874.

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NEW YORK ACADEMY OF MEDICINE.

1874.

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1858. CHARLES E. ISAACS, M. D.
1858. WM. H. VAN BUREN, M. D.
1858. S. CONANT FOSTER, M. D.
1859. JOEL FOSTER, M. D.
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1855. JOHN WATSON, M. D.
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1857. J. MARION SIMS, M. D.
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1859. WILLIAM C. ROBERTS, M. D.
1860. JOHN WATSON, M. D.
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1862. S. CONANT FOSTER, M. D.
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INSTITUTED 1847.

UNA FIDES, ALTARE COMMUNE.

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☞ The figures following the words *Trans.* and *Bul.* denote the number of papers which have been contributed by the Fellows, and printed in the volumes of the "Transactions" and "Bulletin," already published by the Academy.

Those marked thus * have deceased.

MAY, 1874.

ELECTED.

Original. ADAMS, JOHN G., M. D., R. S. 1850–'51; Trust. 1852. C. S., now in office.

1859. AGNEW, CORNELIUS R., M. D., Clinical Professor of the Diseases of the Eye and Ear in the College of Physicians and Surgeons, N. Y.; Surgeon to the Manhattan Eye and Ear Hospital, N. Y. *Bul.* 2.

Original. ANDERSON, JAMES, M. D., P. 1861–'67. Trust. 1861–'74, now in office. *Bul.* 1.

1867. ANDERSON, JAMES H., M. D., Surgeon to the Home for the Aged and Indigent Blind, N. Y.

1864. ANDERSON, WILLIAM C., M. D., Surgeon to S. R. Smith Infirmary, Edgewater, Stapleton, Staten Island, N. Y.

1847. ANDREWS, JARVIS M., M. D.

ELECTED.	
1862.	ARNOLD, E. S. F., M. D., Newport, R. I.
1865.	BAHAN, THOMAS S., M. D., late Physician to the Northern Dispensary, N. Y.
1867.	BALL, A. BRAYTON, M. D., Lecturer on Diseases of the Kidneys in the College of Physicians and Surgeons, N. Y.
1862.	BANKS, JAMES L., M. D., Physician to the Presbyterian Hospital; Consulting Physician to the Presbyterian Home for Aged Women, New York. Trust. 1867, now in office.
1854.	BARKER, B. FORDYCE, M. D., Professor of Clinical Midwifery and Diseases of Women in Bellevue Hospital Medical College; Physician to Bellevue Hospital, N. Y. V. P. 1857. *Trans.* 5; *Bul.* 3.
Original.	* BARKER, LUKE, M. D., *Obit.* 13th December, 1849, *æt.* 59.
1854.	BARRY, ROBERT A., M. D.
1862.	BARSTOW, JOSIAH W., M. D., Physician to Sandford Hall Insane Asylum, Flushing, L. I.
Original.	* BATCHELDER, JOHN P., M. D., late Professor of Surgery in Berkshire Medical Institution, Pittsfield, Mass.; P. 1858; V. P. 1851–'52. *Obit.* 7th April, 1868, *æt.* 82.
1868.	BAYLES, GEORGE, M. D., Physician to Northwestern Dispensary, N. Y.
Original.	BEADLE, EDWARD L., M. D., Vice-President of the College of Physicians and Surgeons, N. Y., late Physician to the New York Lying-in Asylum; V. P. 1853–'57; Trust. 1851; C. S. 1850–'52.
Original.	BEALES, JOHN C., M. D., Consulting Physician to the Presbyterian Home for Aged Women.
Original.	* BEALS, GORHAM, M. D., Physician to the New York Dispensary, *Obit.* 9th January, 1848, *æt.* 29.
1870.	BEARD, GEORGE M., M. D., Physician to the Demilt Dispensary, N. Y. *Bul.* 1.
Original.	* BECK, JOHN B., M. D., Professor of Materia Medica and Medical Jurisprudence in the College of Physicians and Surgeons; V. P. 1847 and 1848. *Obit.* 9th April, 1851, *æt.* 56.
Original.	* BEDFORD, GUNNING S., M. D., Professor of Obstetrics in the University of the City of New York. *Obit.* 5th September, 1870, *æt.* 64.
1862.	BELDEN, EBENEZER B., M. D.
1863.	BELL, AGRIPPA N., M. D., Physician to the Brooklyn City Hospital, Brooklyn, L. I. *Bul.* 2.

Elected.	
1871.	Bell, Christopher M., M. D., Curator of New York Hospital. *Bul.* 1.
1864.	* Bibbins, William B., M. D., Physician to the Demilt Dispensary; Trust. 1867–'71. *Obit.* 16th January, 1871, *æt.* 46.
1871.	Billington, Cornelius E., M. D., Physician to the Demilt Dispensary, N. Y.
1872.	Blake, John Ellis, M. D.
Original.	Blakeman, William N., M. D.
Original.	* Bliss, James C., M. D., V. P. 1850. *Obit.* 12th August, 1855, *æt.* 64.
Original.	Bliven, Jeremiah P., M. D.
Original.	* Blois, Samuel, M. D., *Obit.* 19th October, 1873.
1871.	Blume, Samuel, M. D.
1857.	Blumenthal, Mark, M. D., President of the Institution for Improved Instruction of Deaf Mutes, N. Y. *Bul.* 1.
1873.	Bogert, Cornelius R., M. D., Medical Examiner of the New York Life Insurance Company.
Original.	* Bolton, Jackson, M. D., V. P. 1856; R. S. 1852. *Obit.* 16th February, 1866, *æt.* 51.
1848.	* Booraem, Augustus C., M. D., *Obit.* 16th December, 1871, *æt.* 46.
Original.	* Boyd, Thomas, M. D., *Obit.* 18th March, 1856.
1869.	Bozeman, Nathan, M. D.
1847.	* Brady, Patrick J., M. D., *Obit.* 23d October, 1856, *æt.* 42.
1852.	* Brooks, George W., M. D., *Obit.* 1853.
1863.	Brown, D. Tilden, M. D., Physician to the Bloomingdale Asylum for the Insane, N. Y.
1867.	* Brown, James L., M. D., Physician to the Demilt Dispensary, N. Y. *Obit.* 4th February, 1873, *æt.* 42. *Trans.* 1.
1848.	Brown, William K., M. D., Brooklyn, L. I.
1848.	Brueninghausen, Charles, M. D.
Original.	Buck, Gurdon, M. D., Surgeon to the New York Hospital; Consulting Surgeon to St. Luke's Hospital, N. Y.; C. S. 1848; V. P. 1852, 1856, 1859, 1860. *Trans.* 6; *Bul.* 8.
1855.	Budd, Charles A., M. D., Professor of Obstetrics and the Diseases of Women and Children in the University of the City of New York; Physician to Charity and Mt. Sinai Hospitals, N. Y. *Bul.* 1.
Original.	* Bulkley, Henry D., M. D., Physician to the New York Hospital; P. 1870–'71; V. P. 1862–'68; Trust. 1871. *Obit.* 4th January, 1872, *æt.* 68. *Bul.* 2.

Elected.	
1874.	Bulkley, Lucius D., M. D., Physician to the Union Dispensary, New York.
1848.	* Bullus, Edward S., M. D., *Obit.* 25th September, 1854, *æt.* 50.
1856.	Bumstead, Freeman J., M. D., late Professor of Materia Medica in the College of Physicians and Surgeons, New York. *Bul.* 1.
1861.	Burke, John, M. D.
1865.	Burrall, Frederick A., M. D., Physician to the Presbyterian Home for Aged Women, New York.
1859.	Burtsell, Thomas E., M. D., Physician to St. Vincent Hospital, New York.
1854.	Byrne, John, M. D., Surgeon-in-Chief to St. Mary's Hospital for Women, Brooklyn, L. I. *Bul.* 1.
Original.	* Cameron, James, M. D., *Obit.* 12th December, 1851, *æt.* 66.
1862.	* Cammann, George P., M. D., late Physician to the Demilt Dispensary; Consulting Physician to St. Luke's Hospital, N. Y. *Obit.* 16th February, 1863, *æt.* 59.
Original.	* Campbell, James, M. D., *Obit.* 12th March, 1853, *æt.* 59.
1869.	Caro, Salvatore, M. D.
1858.	* Carrington, William A., M. D., *Obit.* 17th July, 1866.
Original.	* Carter, Galen, M. D., V. P. 1849–'52; Trust. 1851. *Obit.* 2d March, 1870, *æt.* 74.
1869.	Chadsey, Alonzo J., M. D.
1847.	Chalmers, Thomas C., M. D., Ex-surgeon to New York Hospital.
1862.	Chamberlain, William M., M. D., Physician to the Demilt Dispensary, N. Y.; Physician to the Charity Hospital, N. Y.; R. S. 1865–'68.
Original.	* Chapin, John R., M. D., *Obit.* 23d June, 1852, *æt.* 41.
1872.	Chauveau, Jean F., M. D.
1847.	* Cheeseman, John C., M. D., late Surgeon to New York Hospital. *Obit.* 11th November, 1862, *æt.* 75.
1871.	Cheeseman, Timothy Matlack, M. D.
1865.	* Childs, Timothy, M. D., Professor of Anatomy in Bellevue Hospital Medical College. *Obit.* 3d September, 1865, *æt.* 42.
1856.	Church, Allen S., M. D. *Trans.* 1; *Bul.* 1.
1859.	* Churchill, Charles W., M. D., *Obit.* 27th October, 1859.

ELECTED.

1856. CLARK, ALONZO, M. D., LL. D., Professor of Pathology and Practice of Medicine in the College of Physicians and Surgeons, N. Y.; Physician to Bellevue Hospital, N. Y. *Bul.* 2.

1870. CLARK, JAMES G., M. D., W. New Brighton, S. I., N. Y.

Original. CLARKSON, CORNELIUS V., M. D.

Original. CLEMENTS, JAMES W. G., M. D., Physician to the New York Institution for the Blind.

1859. * COCHRAN, GEORGE, M. D., Surgeon to the Brooklyn City Hospital, Brooklyn, L. I. *Obit.* 9th November, 1872, *æt.* 41.

1871. COLES, J. ACKERMAN, M. D., Scotch Plains, N. J.

1854. * CONANT, DAVID S., M. D., Professor of Surgery in the University of Vermont; Surgeon to the Demilt Dispensary, N. Y. *Obit.* 8th October, 1865, *æt.* 40. *Trans.* 1.

1864. * CONNOLLY, JOHN J., M. D., Physician to St. Vincent Hospital, N. Y. *Obit.* 18th June, 1873, *æt.* 36.

Original. * COOPER, JAMES S., M. D., Physician to the Home for Aged and Respectable Indigent Females, N. Y. *Obit.* 11th April, 1867, *æt.* 50.

Original. * COVEL, JOHN C., M. D., Physician to the New York City Prison. *Obit.* 4th November, 1860, *æt.* 64.

1862. * COX, HENRY G., M. D., Professor of the Theory and Practice of Medicine in New York Medical College. *Obit.* 29th May, 1865, *æt.* 47.

1870. CRAMPTON, HENRY E., M. D.

1849. CRANE, JAMES, M. D., Physician to the Brooklyn City Hospital, Brooklyn, L. I.

1847. CRANE, JOHN J., M. D., Surgeon to Bellevue Hospital; Surgeon to the Presbyterian Hospital, N. Y.

1866. CRANE, JOSEPH S., M. D., late Physician to the New York Lying-in Asylum.

1851. * CREVELING, ABRAHAM, M. D., *Obit.* 28th April, 1853, *æt.* 39.

1868. * DALTON, EDWARD B., M. D., late Surgeon of, and Medical Director of U. S. Volunteers. *Obit.* 13th May, 1872, *æt.* 37.

1856. DALTON, JOHN C., M. D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, N. Y.; Orator, 1873; V. P., now in office. *Trans.* 6.

1867. DANA, SAMUEL W., M. D., Physician to New York Dispensary.

ELECTED.

Original. DAVIS, JOHN, M. D.

Original. DELAFIELD, EDWARD, M. D., President of, and Professor Emeritus of the College of Physicians and Surgeons, N. Y.; Consulting Physician to St. Luke's and New York State Woman's Hospitals, N. Y.

1859. DERBY, EDWARD W., M. D., late Physician to the Eastern Dispensary, N. Y.

Original. DETMOLD, WILLIAM, M. D., Professor Emeritus of Military and Clinical Surgery in the College of Physicians and Surgeons, N. Y.; V. P. 1853 to 1856; Orator, 1856. *Trans.* 1; *Bul.* 5.

1860. *DONAGHE, WILLIAM R., M. D., late Lecturer on Surgical Anatomy and Venereal Diseases in the University of the City of New York. *Obit.* 18th July, 1866, *æt.* 36.

1856. DOUGLASS, JOHN H., M. D.

1848. *DOUGLASS, ROBERT, M. D., *Obit.* 25th July, 1861, *æt.* 47.

Original. DOWNS, HENRY S., M. D.

Original. *DRAKE, BENJAMIN, M. D., late President New York County Medical Society, C. S. 1847. *Obit.* 11th January, 1871, *æt.* 65.

1863. DRAPER, JOHN C., M. D., Professor of Chemistry in the University of the City of New York. *Trans.* 1; *Bul.* 1.

1858. DRAPER, WILLIAM H., M. D., Clinical Professor of the Diseases of the Skin in the College of Physicians and Surgeons, N. Y.; Physician to Roosevelt Hospital, N. Y. *Bul.* 6.

1847. DUDLEY, WILLIAM H., M. D., Physician to the Brooklyn City Hospital, Brooklyn, L. I.

1847. *DWIGHT, WILLIAM W., M. D., *Obit.* 11th July, 1861.

1847. *EARLE, EDWARD, M. D., *Obit.* 21st August, 1849.

1851. ELDER, ALEXANDER, M. D., late Physician to the Demilt Dispensary, N. Y.

1858. ELIOT, ELSWORTH, M. D., President of the New York County Medical Society.

1856. ELLIOT, FREDERIC, M. D.

1858. *ELLIOT, GEORGE T., M. D., Professor of Obstetrics in Bellevue Hospital Medical College; Physician to Bellevue Hospital. *Obit.* 28th January, 1871, *æt.* 43. *Trans.* 1; *Bul.* 2.

ELECTED.

1870. ELLIS, HENRY A., M. D.

Original. ELLIS, SAMUEL C., M. D.

1872. ELSBERG, LOUIS, M. D., Clinical Professor of the Diseases of the Throat in the University of the City of New York. *Trans.* 2.

1856. EMMET, THOMAS ADDIS, M. D., Surgeon to the New York State Woman's Hospital, N. Y.

Original. * ENOS, DEWITT C., M. D., Professor of Anatomy in the Long Island Hospital College; Surgeon to the Brooklyn City Hospital, Brooklyn, L. I. *Obit.* 4th December, 1868, *æt.* 48.

1867. FARLEY, JAMES L., M. D., Brooklyn, New York.

1864. FARNHAM, HORACE, P., M. D., late Physician to the Northern Dispensary, N. Y.

Original. * FERGUSON, JOHN T., M. D., *Obit.* 11th October, 1859.

1847. * FERRIS, FLOYD T., M. D., *Obit.* 6th November, 1855, *æt.* 62.

1855. FINNELL, THOMAS C., M. D., late Demonstrator of Anatomy in the University of the City of New York; Surgeon to St. Vincent's Hospital, N. Y. *Bul.* 1.

1847. * FISK, LYMAN, M. D., *Obit.* 1st August, 1859, *æt.* 36.

1847. FITCH, JAMES D., M. D., Consulting Physician to the Colored Home for Indigent and Aged, N. Y.

1862. FLINT, AUSTIN, M. D., Professor of the Principles and Practice of Medicine and Clinical Medicine in Bellevue Hospital Medical College; Physician to Bellevue Hospital, N. Y.; Orator 1868; V. P. 1871 and 1872; P. 1873, and now in office. *Trans.* 2; *Bul.* 3.

1862. FLINT, AUSTIN, Jr., M. D., Professor of Physiology in Bellevue Hospital Medical College; Physician to Bellevue Hospital, N. Y.

1870. FOSTER, FRANK P., M. D., Physician to the New York Dispensary, N. Y. *Trans.* 1.

Original. FOSTER, JOEL, M. D., V. P. 1859 to 1862; Trust. 1862 to 1866.

Original. * FOSTER, SAMUEL CONANT, M. D., late Physician to Bellevue Hospital, N. Y.; R. S. 1855 and 1856; V. P. 1858 and 1859; Orator 1861 and 1862. *Obit.* 18th April, 1873, *æt.* 57. *Trans.* 1.

1856. * FOY, MICHAEL E., M. D., Surgeon of the 38th Regiment New York Volunteers. *Obit.* 9th June, 1861.

ELECTED.

Original. * FRANCIS, JOHN W., M. D., late Professor of Obstetrics and Medical Jurisprudence in Rutgers Medical College, N. Y.; Orator 1847; V. P. 1847; P. 1848. *Obit.* 8th February, 1861, *æt.* 72.

1863. FRANCIS, SAMUEL W., M. D., Newport, R. I.

1871. FRANKELL, EDWARD, M. D., Physician to the New York and Eastern Dispensaries.

1864. FREEMAN, N. MARSTON, M. D., Physician to the Yorkville Dispensary, New York.

1870. FROTHINGHAM, WILLIAM, M. D.

1871. FULLER, ROBERT M., M. D.

1865. FURMAN, G., M. D.

1869. GALLATIN, ALBERT H., M. D., Professor of Analytical Chemistry in Cooper Institute, N. Y. *Trans.* 1.

Original. † GARDNER, A. K., M. D. *Trans.* 3; *Bul.* 2.

Original. GARRISH, JOHN P., M. D., late Physician to the New York Ophthalmic Hospital, N. Y.

1870. GAY, HARVEY S., M. D., late Physician to the New York Lying-in Asylum.

1848. GESCHEIDT, ANTHONY, M. D., Hastings-on-the-Hudson, N. Y.

1863. GILFILLAN WILLIAM, M. D., Professor of Therapeutics and Materia Medica in Long Island Hospital Medical College, Brooklyn, L. I.

1847. * GILFORD, JACOB T., M. D., *Obit.* 11th June, 1869, *æt.* 63.

1856. GOMEZ, HORATIO, M. D., late Physician to the New York Dispensary, N. Y.

1856. GOULEY, JOHN WM. S., M. D., late Professor of Clinical Surgery, and Genito-Urinary Diseases, in the University of the City of New York; Surgeon to Bellevue Hospital, N. Y.

1847. * GRAHAM, JOHN, M. D., *Obit.* 20th May, 1847, *æt.* 47.

1848. * GREEN, DAVID, M. D., *Obit.* 18th October, 1856, *æt.* 60.

Original. * GREEN, HORACE, M. D., Professor of the Theory and Practice of Medicine in New York Medical College. *Obit.* 29th November, 1866, *æt.* 63. *Trans.* 1.

Original. * GREENE, ISAAC, M. D., *Obit.* 3d July, 1854, *æt.* 38.

Original. * GRISCOM, JOHN H., M. D., late Physician to New York Hospital; Orator 1854; V. P. 1854. *Obit.* 28th April, 1874, *æt.* 64. *Trans.* 1; *Bul.* 4.

1872. GRISWOLD, HENRY, M. D.

† Suspended.

ELECTED.	
1847.	* GUERNSEY, PETER B., M. D., Croton Falls, N. Y. *Obit.* 26th November, 1873.
1847.	* GUNN, ALEXANDER N., M. D., late Health Officer of the Port of New York. *Obit.* 20th December, 1871, *æt.* 60.
1867.	HACKLEY, CHARLES E., M. D., Physician to New York Hospital.
Original.	HALL, EDWARDS, M. D.
Original.	HALL, SAMUEL, M. D., late Physician to the New York Dispensary.
Original.	* HALSTED, JONATHAN, M. D., *Obit.* 10th April, 1856, *æt.* 39.
1873.	HAMILTON, ALLAN McL., Physician to New York State Hospital for Diseases of the Nervous System, N. Y.
1864.	HAMILTON, FRANK H., M. D., Professor of Military Surgery in Bellevue Hospital Medical College; Surgeon to Bellevue Hospital, N. Y. *Bul.* 2.
1874.	HANKS, HORACE T., M. D., Physician to the Demilt Dispensary, New York; A. S., now in office.
1857.	HARRIS, ELISHA, M. D., Registrar of Vital Statistics of the Health Department of the City of New York. *Bul.* 3.
1859.	* HARSEN, JACOB, M. D., Trust. 1862. *Obit.* 31st December, 1862, *æt.* 55.
1869.	HART, CHARLES A., M. D.
Original.	* HART, JOHN, M. D., *Obit.* 9th August, 1867, *æt.* 57.
1865.	* HAZLETT, JOHN, M. D., *Obit.* 4th March, 1870, *æt.* 53.
1865.	HEDGES, DAVID A., M. D., Consulting Physician of the Northwestern Dispensary, N. Y.
1871.	HENRY, MORRIS H., M. D., Surgeon-in-Chief to the New York State Emigrants' Hospital, N. Y.
1847.	* HENSCHEL, CHARLES, M. D., late President of the New York Obstetrical Society. *Obit.* 18th September, 1872, *æt.* 62.
1867.	HERRICK, EVERETT, M. D.
1857.	HERZOG, MAX, M. D., late Physician to the German Dispensary, N. Y. *Bul.* 1.
1863.	* HEWIT, HENRY S., M. D., Lecturer on Surgery in the University of the City of New York; Surgeon to Charity Hospital, N. Y. *Obit.* 19th August, 1873, *æt.* 47. *Bul.* 1.
1856.	HEYWOOD, CHARLES F., M. D., Physician to St. Luke's Hospital, N. Y.; R. S. 1857 and 1858.

ELECTED.

1854. LEAMING, JAMES R., M. D., Professor of Principles and Practice of Medicine in the Woman's Medical College, N. Y.; Physician to St. Luke's Hospital, N. Y. *Trans.* 1; *Bul.* 3.

1869. LEE, CHARLES C., M. D., Surgeon to Charity Hospital, N. Y.

1874. LEFFERTS, GEORGE M., M. D., Physician to the Demilt Dispensary, N. Y.

1872. LEO, SIMON N., M. D., Physician to the Home for Aged Hebrews.

1850. * LEO WOLFE, GEORGE, M. D., *Obit.* 14th March, 1855, *æt.* 40.

Original. LEO WOLFE, MORRIS, M. D.

1851. * LEVERIDGE, BENJAMIN C., M. D., *Obit.* 16th April, 1862, *æt.* 63.

1856. LIDELL, JOHN A., M. D., late Surgeon to Bellevue Hospital, N. Y. *Trans.* 1.

Original. LINSLY, JARED, M. D., Consulting Physician to the New York Lying-in Asylum and to the New York Dispensary. Trust. 1865 to 1870.

1854. LITTLE, JAMES L., M. D., Lecturer on Operative Surgery in the College of Physicians and Surgeons, N. Y.; Surgeon to St. Luke's Hospital, N. Y.

1855. LIVINGSTON, WATTS C., M. D., late Physician to Demilt Dispensary, N. Y.

1861. * LOINES, JONAS P., M. D., Vaccine Physician to the Eastern Dispensary, N. Y. *Obit.* 15th December, 1873.

1863. LOOMIS, ALFRED L., M. D., Professor of the Institutes and Practice of Medicine in the University of the City of New York; Physician to Bellevue and Charity Hospitals, N. Y. *Trans.* 1; *Bul.* 3.

1869. LORING, EDWARD G., M. D., Consulting Surgeon to the Brooklyn Eye and Ear Hospital. *Trans.* 1.

1871. LUSK, WILLIAM T., M. D., Professor of Obstetrics and the Diseases of Women in Bellevue Hospital Medical College, N. Y.; Physician to the Bellevue and Charity Hospitals, N. Y.

1847. * LYON, JAMES L., M. D., *Obit.* 24th December, 1858, *æt.* 50.

1864. MACGREGOR, JAMES R., M. D.

1857. * MCALLISTER, GEORGE, M. D., *Obit.* 29th July, 1864, *æt.* 37.

1866. MCCLELLAN, CHRISTOPHER R., M. D.

Original. MCCLELLAND, JOHN, M. D., late Physician to the New York City Lunatic Asylum.

ELECTED.	
Original.	McCREADY, BENJAMIN W., M. D., late Professor of Materia Medica in Bellevue Hospital Medical College.
1847.	* McDONALD, JAMES, M. D., late Physician to Bloomingdale Lunatic Asylum, N. Y. *Obit.* 5th May, 1849, *æt.* 45.
1857.	McLEOD, S. B. WYLIE, M. D., late Physician to the New York Lying-in Asylum.
1865.	McMILLAN, CHARLES, M. D., Physician to the Orphan Asylum of the Protestant Episcopal Church, N. Y.
1847.	* McNEVEN, WILLIAM H., M. D., late Physician to the New York Dispensary, N. Y. *Obit.* 12th May, 1854, *æt.* 38.
1848.	McNULTY, JOHN, M. D.; late Surgeon U. S. Volunteers. *Bul.* 1.
Original.	* MANLEY, JAMES R., M. D., late Lecturer on Obstetrics in the College of Physicians and Surgeons, N. Y.; V. P. 1849; Orator 1848. *Obit.* 21st November, 1857, *æt.* 70.
Original.	MARKOE, THOMAS M., M. D., Professor of Surgery in the College of Physicians and Surgeons, N. Y.; Surgeon to Bellevue and Roosevelt Hospitals, N. Y.; L. 1847. *Bul.* 1.
1847.	* MARTIN, JOSEPH, M. D., *Obit.* 26th April, 1864. *Bul.* 2.
1872.	MARTIN, T. DWIGHT, M. D.
Original.	* MARVIN, DANIEL D., M. D., *Obit.* 21st October, 1852, *æt.* 40.
1872.	MASON, JOHN J., M. D.
1847.	MAXWELL, WILLIAM H., M. D., Consulting Surgeon to the New York Dispensary.
Original.	* MEIKLEHAM, DAVID S., M. D., *Obit.* 20th November, 1849.
Original.	METCALFE, JOHN T., M. D., Professor of Clinical Medicine in the College of Physicians and Surgeons, N. Y.; Consulting Physician to St. Luke's and Roosevelt Hospitals, N. Y. *Trans.* 1; *Bul.* 2.
1848.	* MILLER, JOHN, M. D., *Obit.* 13th January, 1863, *æt.* 56.
1848.	* MILLER, WM. ELLISON, M. D., *Obit.* 11th January, 1852, *æt.* 48.
1848.	* MINER, WILLIAM, M. D., *Obit.* 16th November, 1859, *æt.* 45.
Original.	* MINER, WILLIAM W., M. D., V. P. 1848. *Obit.* 20th March, 1863, *æt.* 83.
1847.	MITCHELL, CHAUNCEY L., M. D., late Professor of Obstetrics in Castleton Medical College, Vt.; Consulting Physician to St. Mary's Hospital, Brooklyn, L. I.

Elected.	
1852.	Monell, Joseph A., M. D.
1871.	Monell, Joseph S., M. D.
1848.	*Moore, Samuel W., M. D., late Physician to the N. Y. Hospital. *Obit.* 25th August, 1854, *æt.* 68.
1849.	*Moran, Thomas, M. D., *Obit.* 1853.
1870.	Morris, Moreau, M. D., late Sanitary Inspector of the Health Department of the City of New York.
1870.	Morris, Stuyvesant F., M. D., Sanitary Inspector of the Health Department of the City of New York.
1869.	Morton, Jeremiah C., M. D.
1874.	Mosher, Jacob, M. D., Tompkinsville, S. I.
Original.	*Mott, Valentine, M. D., LL. D., Professor of Surgery in the University of the City of New York; Consulting Surgeon to New York and Bellevue Hospitals; P. 1849 and 1857. *Obit.* 26th April, 1865, *æt.* 79. *Trans.* 4.
1871.	Neftel, William B., M. D.
1848.	*Neilson, John, M. D., *Obit.* 19th June, 1857, *æt.* 82.
1852.	Nelson, James B., M. D.
1874.	Nesmith, Robert D., M. D.
1847.	Nichols, Elias S., M. D.
1859.	Nichols, Truman, M. D.
1873.	Nicoll, Henry D., M. D., Physician to the Northern Dispensary, N. Y.
1861.	Noeggerath, Emil, M. D., late Professor of Clinical Midwifery in New York Medical College; Physician to the German Hospital, N. Y. *Bul.* 2.
1871.	*Nott, Josiah Clark, M. D., late Professor of Surgery in Mobile Medical College. *Obit.* 31st March, 1873, *æt.* 69.
1862.	Noyes, Henry D., M. D., Professor of Ophthalmology and Otology in Bellevue Hospital Medical College, N. Y.; Surgeon to the New York Eye and Ear Infirmary. *Bul.* 2.
Original.	*Ogden, Benjamin, M. D., late Physician to the Bloomingdale Asylum for the Insane; Consulting Physician to St. Luke's Hospital, N. Y.; Trust. 1853 to 1859, 1861. *Obit.* 18th June, 1867, *æt.* 70.
1862.	Ordronaux, John, M. D., late Professor of Medical Jurisprudence in Law School of Columbia College; Orator 1865.
1873.	Orton, Samuel H., M. D.

ELECTED.

1861. OTIS, FESSENDEN N., M. D., Professor of Venereal Diseases in the College of Physicians and Surgeons, N. Y.

1870. O'MEAGHER, WILLIAM, M. D., late Physician to the New York Dispensary.

1857. *O'REILLY, JOHN, M. D., *Obit.* 6th December, 1868, *æt.* 56. *Trans.* 1.

1855. O'SULLIVAN, RICHARD J., M. D., Consulting Physician to the Eastern Dispensary, N. Y.

1871. PACKARD, CHARLES W., M. D., Physician to St. Luke's Hospital, N. Y.

1864. PAINE, MARTYN, M. D., LL. D., Emeritus Professor of the Institutes of Medicine and Materia Medica in the University of the City of New York.

1857. PALMER, LUCIUS N., M. D., Physician to the Williamsburgh Dispensary, Brooklyn, L. I.

1869. PARDEE, CHARLES I., M. D., Lecturer on Diseases of the Eye and Ear in the University of the City of New York.

1873. PARIGOT, JULIUS, M. D. *Bul.* 2.

Original. PARKER, WILLARD, M. D., Professor of Clinical Surgery in the College of Physicians and Surgeons, N. Y.; Consulting Surgeon to the New York, Bellevue, and Presbyterian Hospitals, N. Y.; Trust. 1851; V. P. 1853; P. 1856. *Bul.* 2.

1847. *PARKINSON, WILLIAM B., M. D., Physician to the New York Dispensary. *Obit.* 11th May, 1856, *æt.* 45.

1847. *PAUL, JAMES C., M. D., *Obit.* 5th May, 1859.

1858. PEASLEE, EDMUND R., M. D., LL. D., late Professor of Anatomy in the New York Medical College; Professor of the Diseases of Women in Albany Medical College, N. Y.; Surgeon to the New York State Woman's Hospital, N. Y.; Orator 1858; V. P. 1868 to 1871; P. 1871 and 1872. *Trans.* 3; *Bul.* 5.

Original. *PENNELL, RICHARD, M. D., *Obit.* 11th April, 1861, *æt.* 62.

1850. PETERS, GEORGE A., M. D., Surgeon to St. Luke's Hospital, N. Y.; A. S. 1852.

1870. PETERS, JOHN C., M. D., President of the New York Medical Journal Association. *Trans.* 1.

1874. PEUGNET, EUGENE, M. D.

Original. *PHILLIPS, SAMUEL B., *Obit.* 3d March, 1857, *æt.* 54.

Original. *PIATT, WILLIAM F., M. D., *Obit.* 6th May, 1848, *æt.* 42.

1867. PINCKNEY, HOWARD, M. D., Assistant Surgeon to the New York Eye and Ear Infirmary, N. Y.

Elected.	
1873.	Polk, William M., M. D.
1865.	Pomeroy, Oren D., M. D., Surgeon to the Manhattan Eye and Ear Hospital, N. Y.; Physician to the Northern Dispensary, N. Y.
Original.	Pond, James O., M. D., T. 1848 to 1874, now in office.
1867.	Pooley, Thomas R., M. D., Surgeon to Riverside Hospital, Yonkers, N. Y.; Surgeon to Charity Hospital, N. Y.
1856.	*Porter, Mortimer G., M. D., *Obit.* 24th November, 1863, *æt.* 37. *Bul.* 1.
1847.	Post, Alfred C., M. D., Professor of Surgery in the University of the City of New York; Consulting Surgeon to the New York Hospital; Orator 1849; V. P. 1861 to 1866; P. 1869. *Bul.* 4.
1870.	Post, William, H. B., M. D., Inspector of the Health Department of the City of New York; A. S. 1871 to 1874.
Original.	*Power, William, M. D., *Obit.* 14th September, 1858, *æt.* 60.
Original.	*Pratt, Peter, M. D., *Obit.* 1860, *æt.* 52.
1861.	Prince, Christopher, M. D., late Surgeon New York Police. *Bul.* 2.
1871.	Purdy, Alfred E. M., M. D., late Surgeon New York Police.
Original.	Purdy, Alfred S., M. D.
1866.	Purdy, James W., M. D.
Original.	Purdy, Samuel A., M. D., R. S. 1853 and 1854.
Original.	Purple, Samuel S., M. D., Honorary Member of the Medical Society of the State of New York; late Physician to the New York Dispensary; V. P. 1872 to 1874, now in office. *Trans.* 1.
1847.	Putnam, Frederick A., M. D.
1851.	Randolph, Israel, M. D.
1850.	Ranney, Evander W., M. D.
1863.	Ranney, Henry D., M. D.
1859.	Ranney, Lafayette, M. D.
1851.	*Ranney, Moses H., Physician to the New York City Lunatic Asylum. *Obit.* 7th December, 1864, *æt.* 50. *Bul.* 1.
1856.	Raphael, Benjamin J., M. D., late Professor of Surgery in New York Medical College; Surgeon to Mount Sinai Hospital, N. Y.

ELECTED.	
1859.	*RAY, ROBERT, Jr., M. D., *Obit.* 3d July, 1860, *æt.* 27.
Original.	*REESE, DAVID MEREDITH, M. D., late Professor of the Principles of Surgery in Castleton Medical College. *Obit.* 13th May, 1861, *æt.* 60.
1872.	REINFELDER, MAX J., M. D.
1866.	REYNOLDS, JAMES B., M. D., Physician to the Nursery and Child's Hospital, N. Y.
1855.	RICHARDS, JOSEPH W., M. D.
1874.	ROBERTS, NATHAN S., M. D.
Original.	*ROBERTS, WILLIAM C., late Physician to the Northern Dispensary, N. Y.; Orator 1859, V. P. 1870 to 1873. *Obit.* 9th December, 1873, *æt.* 63. *Bul.* 6.
Original.	*ROBESON, ABEL B., Physician to Bellevue Hospital, N. Y. *Obit.* 22d March, 1853, *æt.* 36.
1872.	ROBIE, JOHN W., M. D., Physician to the Masonic Board of Relief, New York.
1869.	ROCKWELL, ALPHONZO D., M. D. *Bul.* 1.
1873.	RODENSTEIN, CHARLES F., M. D., Physician to the New York Catholic Protectory, Fordham, N. Y.
1862.	RODENSTEIN, LOUIS A., M. D.
Original.	*RODGERS, JOHN KEARNY, M. D., Surgeon to the New York Hospital; V. P. 1848 to 1851; Trust. 1851. *Obit.* 9th November, 1851, *æt.* 59.
1847.	*ROGERS, J. SMYTH, M. D., late Professor of Materia Medica in the New York College of Pharmacy. *Obit.* 29th March, 1851, *æt.* 57.
1867.	ROGERS, STEPHEN, M. D., Surgeon to the Demilt Dispensary, N. Y. *Bul.* 2.
1872.	ROOF, STEPHEN W., M. D.
1865.	ROOSA, D. B. ST. JOHN, M. D., Clinical Professor of the Diseases of the Eye and Ear in the University of the City of New York; Surgeon to the Manhattan Eye and Ear Hospital, N. Y. *Bul.* 1.
1862.	ROSENBERG, EMIL, M. D., Physician to the German Dispensary, N. Y.
1858.	ROSS, JAMES, M. D., late Physician to the Northern Dispensary, N. Y.
1871.	RUSSEL, CHARLES P., M. D., Registrar of Vital Statistics of the Health Department of the City of New York. *Trans.* 2.
Original.	SABINE, GUSTAVUS A., M. D., Consulting Physician to the New York State Woman's Hospital, N. Y.
1873.	SATTERLEE, F. LE ROY, M. D., Professor of Chemistry,

ELECTED.

Materia Medica, and Therapeutics in the New York College of Dentistry.

1864. SATTERLEE, RICHARD S., M. D., Brigadier-General U. S. Army.

Original. SAYRE, LEWIS A., M. D., Professor of Orthopedic Surgery in Bellevue Hospital Medical College, N. Y.; Surgeon to Bellevue and Charity Hospitals, N. Y. *Bul.* 2.

1852. * SCHILLING, ERNEST, M. D., Physician to New York State Emigrant Hospital, N. Y. *Obit.* 25th April, 1872, *æt.* 63.

Original. * SCHMIDT, JOHN W., Jr., M. D., Surgeon to St. Vincent's Hospital, N. Y. *Obit.* 1857, *æt.* 50.

1847. SCHRIMER, WILLIAM, M. D.

1873. SCHULTZE, LOUIS F., M. D.

1873. SEGUIN, EDWARD C., M. D., Physician to the Presbyterian Hospital, N. Y.; Lecturer on Diseases of the Nervous System in the College of Physicians and Surgeons, N. Y.

1870. SELL, EDWARD H. M., M. D.

1856. * SEWELL, JOHN G., M. D., late Physician to the Northwestern Dispensary, N. Y.; Ch. of Section on Obstetrics. *Obit.* 18th January, 1874, *æt.* 51.

Original. * SHANKS, JOHN, M. D., *Obit.* 10th August, 1870, *æt.* 70.

1862. * SHEPPARD, JOHN W., M. D., *Obit.* 5th October, 1868, *æt.* 46.

Original. * SHERWOOD, BURRITT, M. D., *Obit.* 10th August, 1854, *æt.* 53.

1856. SIMS, J. MARION, M. D., Orator 1857; Surgeon to New York State Woman's Hospital, N. Y. *Bul.* 2.

Original. * SMITH, DAVID, M. D., *Obit.* 17th January, 1867, *æt.* 57.

1870. * SMITH, DAVID A., M. D., *Obit.* 5th April, 1872, *æt.* 29.

Original. * SMITH, GILBERT, M. D., *Obit.* 16th July, 1851, *æt.* 80.

1858. SMITH, GOUVERNEUR M., M. D., Physician to the New York Hospital; L. 1861, 1862; Orator 1869. *Trans.* 4; *Bul.* 3.

1864. SMITH, HANBURY, M. D.

1853. SMITH, JAMES O., M. D.

1867. SMITH, JEROME C., M. D., Physician to the Northeastern Dispensary, N. Y.

Original. * SMITH, JOSEPH MATHER, M. D., Professor of the Theory and Practice of Medicine in the College of Physicians and Surgeons, N. Y.; Physician to New York Hospital; Orator 1850; V. P. 1850 and 1851; P. 1854. *Obit.* 22d April, 1861, *æt.* 77. *Bul.* 2.

ELECTED.

1856. SMITH, J. LEWIS, M. D., Physician to Charity and Infant Hospitals, N. Y. *Trans.* 2.

1866. SMITH, OSCAR G., M. D.

1855. SMITH, STEPHEN, M. D., late Professor of Anatomy in Bellevue Hospital Medical College, N. Y.; Surgeon to Bellevue Hospital; Orator 1867. *Bul.* 1.

1870. SNELLING, FREDERICK G., M. D.

1847. * SNOWDEN, JOHN, M. D., *Obit.* 22d January, 1848.

1864. SPEIR, S. FLEET, M. D., Surgeon to the Brooklyn City Hospital, Brooklyn, L. I.

1869. SPRENG, JUSTUS J., M. D.

Original. * SPRING, EDWARD, M. D., *Obit.* 12th February, 1850.

1859. SQUIBB, EDWARD R., M. D., Brooklyn, L. I. *Bul.* 4.

Original. * STEARNS, JOHN, M. D., P. 1847. *Obit.* 17th March, 1848. *æt.* 78.

Original. * STEPHENSON, MARK, M. D., Physician to the Ophthalmic Hospital, N. Y. *Obit.* 28th August, 1865, *æt.* 62.

Original. * STEVENS, ALEXANDER H., M. D., LL. D., Emeritus Professor of Surgery in the College of Physicians and Surgeons, N. Y.; Consulting Surgeon to New York Hospital; P. 1851. *Obit.* 30th March, 1869, *æt.* 79.

1847. * STEWART, JAMES, M. D., Consulting Physician to the Northern Dispensary, N. Y. *Obit.* 12th September, 1864, *æt.* 65. *Trans.* 1.

1847. * STICKNEY, JOSIAH DWIGHT, M. D., *Obit.* 30th September, 1849, *æt.* 34.

1865. * STILES, R. CRESSON, M. D., Professor of Physiology in Berkshire Medical Institution, Mass.; Consulting Physician to Kings County Hospital, L. I. *Obit.* 17th April, 1873, *æt.* 42.

Original. * STIMPSON, EDWIN B., M. D., late Physician to the New York Lying-in Asylum. *Obit.* 15th May, 1858 *æt.* 36.

1865. STIRLING, THOMAS B., M. D.

Original. STONE, JOHN O., M. D., late Surgeon to Bellevue Hospital, N. Y. *Bul.* 1.

Original. STORER, EBENEZER, M. D.

1859. SWAN, CHARLES Y., M. D.

Original. * SWEENEY, HUGH, M. D., *Obit.* 15th September, 1857, *æt.* 52.

1851. * SWEENEY, JAMES, M. D., *Obit.* 1872.

1847. * SWETT, JOHN A., M. D., Professor of the Theory and Practice of Medicine in the University of the City of

ELECTED.

New York; Physician to the New York Hospital; Orator 1853. *Obit.* 18th September, 1854, *æt.* 45.

1866. SWIFT, FOSTER, M. D., late Professor of Obstetrics and the Diseases of Women and Children in Long Island Hospital College, Brooklyn, L. I.

Original *TAFT, MARCUS L., M. D., A. S. 1848. *Obit.* 8th February, 1850, *æt.* 29.

1872. TAUZSKY, RUDOLPH, M. D.

1867. TAYLOR, CHARLES F., M. D., Surgeon to the Orthopædic Dispensary, N. Y. *Bul.* 2.

Original. TAYLOR, ISAAC E., M. D., President of, and Emeritus Professor of Obstetrics and the Diseases of Women in Bellevue Hospital Medical College, N. Y.; Physician to Bellevue and Charity Hospitals; V. P. 1857 and 1858; Trust. 1872, now in office. *Bul.* 3.

1862. TEATS, SYLVESTER, M. D.

1848. TELLCAMPF, THEODORE S., M. D., late Physician-in-Chief to New York State Emigrants' Hospital.

1865. TELLER, SELIGMAN, M. D., late Physician to Mount Sinai Hospital, N. Y.

1847. *THAYER, HENRY W., M. D., *Obit.* 1857.

1859. THEBAUD, JULIUS S., M. D., Surgeon to St. Vincent's Hospital, N. Y.

1857. THOMAS, T. GAILLARD, M. D., Professor of Obstetrics and the Diseases of Women and Children in the College of Physicians and Surgeons, N. Y.; Physician to Bellevue and Roosevelt Hospitals, N. Y.; Surgeon to the New York State Woman's Hospital, N. Y.; R. S. 1858 to 1861. *Trans.* 2; *Bul.* 1.

1869. THOMPSON, GEORGE, M. D., Surgeon to the New York Dispensary.

1864. THOMS, WILLIAM F., M. D., Statistical Secretary 1868 to 1873. *Bul.* 1.

1864. THOMSON, WILLIAM H., M. D., Professor of Materia Medica and Therapeutics in the University of the City of New York; Physician to Charity Hospital, N. Y. *Bul.* 2.

1867. *TOWNSEND, JOHN F., M. D., *Obit.* 8th January, 1874, *æt.* 64.

Original. *TOWNSEND, PETER S., M. D., *Obit.* 26th March, 1849, *æt.* 54.

1857. TUCKER, CHARLES P., M. D., Physician to the Home for Friendless Women, New York.

1863. *TUCKER, GEORGE H., M. D., *Obit.* 25th January, 1862, *æt.* 35.

1854. *TUTTLE, JOHN T., M. D., *Obit.* 27th January, 1870, *æt.* 68.

ELECTED.

1854. *UHL, DAVID, M. D., *Obit.* 17th September, 1858, *æt.* 36.

Original. *UNDERHILL, ALFRED, M. D., V. P. 1863 to 1866; Trust. 1866 to 1873. *Obit.* 7th December, 1873, *æt.* 64. *Bul.* 2.

1847. *VACHE, ALEXANDER F., M. D., Physician to the Marine Hospital, S. I. *Obit.* 9th June, 1857, *æt.* 57.

Original. *VAN ARSDALE, HENRY, M. D., Morristown, N. J. *Obit.* 25th January, 1864.

Original. VAN ARSDALE, HENRY, M. D.

1847. *VAN ARSEDALE, PETER, M. D. *Obit.* 1858.

1856. *VAN BEUREN, PETER, M. D., *Obit.* 5th December, 1873, *æt.* 71.

Original. *VAN BEUREN, THOMAS, M. D., *Obit.* 1848.

Original. VAN BUREN, WILLIAM H., M. D., Professor of the Principles of Surgery and Diseases of the Genito-Urinary Organs in Bellevue Hospital College, N. Y.; Consulting Surgeon to New York Hospital; Consulting Surgeon to Bellevue Hospital, N. Y.; V. P. 1858. *Trans* 3; *Bul.* 1.

1859. VAN DOREN, MATHEW D., M. D.

Original. VAN KLEECK, JOHN R., M. D., late Pres. of the New York County Medical Society; Trust. 1861 to 1866.

1847. VAN PELT, MOSES D., M. D., V. P. 1860 to 1864; Trust. 1864 to 1869.

1847. VAN WINKLE, EDWARD H., M. D.

1847. VANDERPOEL, EDWARD, M. D.

1859. *VANDERVEER, JACOB H., M. D., *Obit.* 20th August, 1873, *æt.* 55.

Original. VANDERVOORT, JOHN L., M. D., R. S. 1849.

1847. VARICK, THEODORE R., M. D., late President of New Jersey State Medical Society, Jersey City, N. J.

1862. *VEDDER, JOSEPH H., M. D., *Obit.* 18th July, 1864, *æt.* 33.

1854. *VON ROTH, WOLDEMAN, M. D., *Obit.* 1857.

1870. WALSER, THEODORE, M. D., late Deputy Health-Officer of the Port of New York, New Brighton, S. I. *Trans.* 1.

1873. WARD, EDWIN F., M. D., Physician to the Demilt Dispensary, N. Y.

1853. WARNER, EVERARDUS B., M. D., Physician to the Northern Dispensary, N. Y.

Elected.	
Original.	* Washington, James A., M. D., *Obit.* 30th August, 1847, *æt.* 45.
1853.	* Watson, John, M. D., Surgeon to the New York Hospital; P. 1859 and 1860; Orator 1855 and 1860. *Obit.* 3d June, 1863. *Trans.* 1; *Bul.* 1.
Original.	* Watts, Robert, Jr., M. D., Professor of Anatomy in the College of Physicians and Surgeons, N. Y.; T. 1847. *Obit.* 8th September, 1867, *æt.* 55.
1867.	Weber, Leonard, M. D.
Original.	Weeks, Cyrus, M. D.
1866.	Weir, Robert F., M. D., Surgeon to St. Luke's Hospital; Surgeon to Roosevelt Hospital, N. Y.; Surgeon to the New York Eye and Ear Infirmary.
1870.	Weisse, Faneuil D., M. D., Professor of Dermatology in the University of the City of New York.
Original.	Wells, Ovid P., M. D.
Original.	* White, Ambrose L., M. D., late Physician to the Eastern Dispensary, N. Y. *Obit.* 2d June, 1865, *æt.* 61.
1858.	White, Francis V., M. D., Physician to the Eastern Dispensary, N. Y.
Original.	White, Oliver, M. D., Consulting Physician to the Presbyterian Hospital, N. Y.; V. P. 1866 to 1870; Trust., now in office.
Original.	* White, Samuel P., M. D., late Professor of Surgery in the Berkshire Medical Institution; Trust. 1853 to 1858. *Obit.* 6th June, 1867, *æt.* 66.
1867.	White, William T., M. D., Surgeon to the Demilt Dispensary, N. Y.; R. S. 1871 to 1874, now in office.
1869.	Whitehead, William R., M. D., Physician to the Northwestern Dispensary, N. Y.
1847.	Wilkes, George, M. D., Consulting Surgeon to the New York Eye and Ear Infirmary.
Original.	* Williams, Merrill W., M. D., *Obit.* 3d December, 1873, *æt.* 72.
1860.	* Winchell, Martin E., M. D., *Obit.* 1st May, 1864, *æt.* 35. *Bul.* 1.
1869.	Winston, Gustavus S., M. D., late Physician to the Demilt Dispensary, N. Y.
Original.	* Wood, Isaac, M. D., Consulting Physician to Bellevue Hospital, N. Y.; P. 1850 and 1853; V. P. 1849; Trust. 1851 and 1852; 1859 to 1863. *Obit.* 25th March, 1868, *æt.* 74.

ELECTED.	
Original.	WOOD, JAMES R., M. D., LL. D., Emeritus Professor of Surgery in Bellevue Hospital Medical College, N. Y.; Surgeon to Bellevue and Charity Hospitals, N. Y.; V. P. 1857.
Original.	WOOD, STEPHEN, M. D.
1857.	WOODHULL, HENRY W. B., M. D.
1852.	* WOODWARD, GEORGE F., M. D. *Obit.* 1857.
1871.	WOOLLEY, JAMES V. S., M. D., Physician to the Presbyterian Home for Aged Women, N. Y.
Original.	WOOSTER, JOSEPH, M. D.
1869.	WRIGHT, CHARLES, M. D.
1873.	YALE, LE ROY M., M. D., Surgeon to Charity Hospital, N. Y.

NON-RESIDENT FELLOWS.

*Berger, Francis E., M. D., Paris, France.

Carson, John W., M. D., late Physician to the New York Dispensary, Canada.

Draper, John W. M. D., LL. D., President of, and Professor Emeritus of Chemistry in Medical Department of the University of the City of New York.

Dunster, Edward S., M. D., Professor of Midwifery in and Diseases of Women in Vermont Medical College.

Hepburn, James C., M. D., Japan.

*Johnston, Frank U., M. D., late Consulting Physician to New York Hospital, Cooperstown, N. Y. *Obit.* 7th January, 1858, *æt.* 61.

*Lee, Charles Alfred, M. D., Emeritus Professor of Materia Medica and Hygiene in Buffalo Medical College, Peekskill, N. Y. *Obit.* 14th February, 1872, *æt.* 71.

Lewis, William B., M. D., Florida.

Merritt, J. King, M. D., Flushing, L. I.

North, Nelson J., M. D., South Carolina.

Olmstead, Roger S., M. D., Nebraska.

Sands, Austin L., M. D.

Shrady, George F., M. D., Barrytown, N. Y.

Sloan, William J., M. D., U. S. A.

Stewart, F. Campbell, M. D.

Thompson, Bradford S., M. D., Salisbury, Conn.

*Van Arsdale, Henry, M. D., Morristown, N. J. *Obit.* 25th January, 1864.

Vermilye, William E., M. D., Pittsfield, Mass.

HONORARY FELLOWS.

Elected.	
1871.	Bowditch, Henry I., M. D., Professor of Clinical Medicine in Harvard University, Boston, Mass.
1859.	* Gibson, William, M. D., Professor of the Principles and Practice of Surgery in the University of Pennsylvania, Philadelphia, Pa.
1857.	* Ives, Eli, M. D., Professor of Materia Medica and Botany in Yale College, New Haven, Conn.
1860.	* Jackson, James, M. D., Professor Emeritus of the Practice of Physic in the University of Cambridge, Boston.
1859.	* La Roche, Rena, M. D., of Philadelphia, Pa.
1859.	* Mussey, Reuben D., M. D., Professor of Surgery in the Medical College of Ohio, Cincinnati, O.
1857.	* Spaulding, Mathias, M. D., of Amherst, Mass.
1871.	Stillé, Alfred, M. D., Professor of Clinical Medicine in the University of Pennsylvania, Philadelphia, Pa.
1874.	Vanderpoel, S. Oakley, M. D., Health Officer of the Port of New York.
1871.	Wood, George B., M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania, Philadelphia, Pa.

CORRESPONDING FELLOWS.

ELECTED.

1873. ACKLAND, HENRY W., M. D., F. R. S., Regius Professor of Medicine in the University of Oxford, England.

1856. *ADAMS, FRANCIS, M. D., LL. D., Surgeon, Banchory, Scotland.

1847. *AGASSIZ, LOUIS JOHN R., M. D., F. R. S., Professor of Natural History in Harvard University of Cambridge, Mass.

1850. *AMUSSAT, JEAN ZUILME, M. D., Member of the Royal Academy of Medicine, Paris, France.

1854. ANDRAL, GABRIEL, M. D., Professor of Pathology in the Faculty of Medicine, Paris, France.

1854. BARTHEZ, ERNST, M. D., Paris, France.

1847. *BECK, THEODRICK ROMEYN, M. D., Professor of Medical Jurisprudence in Albany Medical College.

1857. BENNETT, JOHN HUGHS, M. D., Professor of Medicine in the University of Edinburgh, Scotland.

1854. BIGELOW, JACOB, M. D., Professor of Materia Medica in Harvard University, Cambridge, Mass.

1847. *BLATCHFORD, THOMAS W., M. D., Troy, N. Y.

1867. BROWN-SÉQUARD, C. E., M. D., Paris, France.

1871. CHAMBERS, THOMAS K., M. D., Physician to and Lecturer on Medicine at St. Mary's Hospital, London.

1847. *CIVALE, JEAN, M. D., Honorary Member of the Academy of Medicine, Paris, France.

1866. CUNHA, JOSÉ DI, M. D., Rio Janeiro, Brazil.

1868. DAVIS, NATHAN S., M. D., Professor of the Principles and Practice of Medicine in Chicago Medical College.

1872. DICHIARA, FRANCESCO, M. D., Palermo, Italy.

ELECTED.

1850. * DICKSON, SAMUEL H., M. D., Professor of the Institutes of Medicine in South Carolina Medical College.

1871. DICKINSON, WILLIAM H., M. D., Physician to and Lecturer on Pathology at St. George's Hospital, London, England.

1854. * DUBOIS, Baron PAUL, Dean and Professor of Clinical Midwifery in the Faculty of Medicine of Paris, France.

1867. DUMONT, HENRI, M. D., Havana, Cuba.

1848. DUPIERRIS, MARTIAL, M. D., Havana, Cuba.

1850. * FENNER, ERASMUS D., M. D., Professor of the Theory and Practice of Medicine in New Orleans School of Medicine.

1849. FERGUSON, Sir WILLIAM, F. R. S., Surgeon to King's College Hospital, London, England.

1851. GROSS, SAMUEL D., M. D., Professor of Surgery in the Medical Department of Louisville University, Louisville, Ky.

1854. GUGGENBUHL, J., M. D., Paris, France.

1847. * HARRIS, THOMAS, M. D., Surgeon-General U. S. Navy, Washington, D. C.

1848. * HOLLAND, Sir HENRY, Bart., M. D., D. C. L., LL. D., F. R. S., Physician to H. M. the Queen, London, England.

1850. * HOOKER, WORTHINGTON, M. D., Professor of the Theory and Practice of Medicine in Yale College, New Haven, Conn.

1874. JENNER, Sir WILLIAM, Bart., M. D., D. C. L. F. R. S., Professor of Clinical Medicine in University College, London, England.

1853. * LEROY DE ETIOLLES, JEAN J. J., M. D., Paris, France.

1871. * NÉLATON, AUGUSTE, Professor of Clinical Surgery in the University of Paris, France.

1874. OWEN, Sir RICHARD, M. D., Hunterian Professor in the Royal College of Surgeons, London, England.

1874. PAGET, Sir JAMES, Bart., M. D., F. R. S., D. C. L., Consulting Surgeon to St. Bartholomew's Hospital, London, England.

1857. PEASLEE, EDMUND R., M. D., Professor of Surgery in Dartmouth Medical College, Hanover, N. H.

1853. PRINCE VIROMMA LUANG SI TIRAT SANIK, Siam.

1868. POST, GEORGE E., M. D., Beirut, Syria.

1849. * REOFREY, BUREAUD DE, M. D.

Elected.	
1850.	Reynolds, Edward, M. D., Boston, Mass.
1852.	Ricord, Philippe, M. D., Member of the Royal Academy of Medicine, Paris, France.
1854.	Rilliet, François, M. D., Paris, France.
1860.	Rosser, D. P., M. D., Athens, Greece.
1849.	* Roget, Peter Mark, M. D., F. R. S., Professor of Physiology in the Royal College, London, England.
1871.	Rokitansky, Carl, M. D., Professor of Pathology in the University of Vienna.
1856.	* Simpson, James Y., M. D., Professor of Midwifery in the University of Edinburgh, Scotland.
1848.	* Smith, Ashbel, M. D., Texas.
1872.	Tilt, Edward J., M. D., Consulting Physician to the Farringdon General Dispensary, London, England.
1852.	Townsend, James C., M. D., Long Island, N. Y.
1852.	* Velpeau, Alfred A. L. M., Professor of Clinical Surgery in the Faculty of Medicine of Paris, Surgeon to the Hôpital de la Charité, Paris.
1871.	Virchow, Rudolph, M. D., Professor of Pathological Anatomy in the University of Berlin.
1869.	Voss, Lothar H., M. D., of Berleberg, Prussia.
1857.	* Wing, Joel A., M. D., late President New York State Medical Society, Albany, N. Y.
1867.	Wortabet, John, M. D., Beirut, Syria.

CONTENTS.

THE TRANSACTIONS

OF THE

NEW YORK ACADEMY OF MEDICINE.

STATED MEETING, MARCH 16, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DR. WM. B. NEFTEL was elected a Resident Fellow.

DR. T. G. THOMAS then read the paper for the evening, entitled "Diagnosis of Cancer of the Ovaries" (published in the *Transactions*).

Dr. E. Noeggerath said the paper is one of importance, as it helps to fill a gap.

He had seen twelve cases of cancer of the ovaries, and coincided with the author of the paper in the views therein expressed. He would suggest two symptoms that he considered characteristic: 1st, an infiltration and hardening of the tissues of the vesico-vaginal septum; 2d, an infiltration of the glands of the abdomen and omentum.

The lower part of the cyst is often adherent to the brim of the pelvis.

One peculiar kind of cancer of the ovary, which develops inside of large cysts, cannot be recognized till after tapping.

In one case that he tapped he could get no fluid with an ordinary-sized trocar, but with one of large size drew off a substance resembling sausage-meat; that case proved to be one of carcinoma of the ovary.

In regard to Dr. Thomas's second case, he, at the time, thought he had to deal with a case of malignant disease of the ovary.

Dr. Chamberlain inquired of Dr. Noeggerath if, in puncturing the tumor with a large trocar, the hemorrhage had been great.

Dr. Noeggerath, in reply, said that the hemorrhage was not great, as he had probably struck a portion of the wall that did not contain a large number of vessels.

Dr. Chadsey remarked that such an accident would involve the necessity of removing the tumor.

Dr. W. C. Roberts said the paper throws a good deal of light on the subject of ovariotomy. The diagnosis is very important, and, if made out early, a less number of operations would be made.

Dr. Spier thought that these cases might be examined with the microscope previous to an operation, and inquired of Dr. Thomas if he was in the habit of removing portions of these tumors for microscopic examination. Dr. Thomas replied that he was not.

Dr. Hart mentioned a case that occurred in practice of Dr. Nelson, where the woman died suddenly, and on post-mortem examination, the tumor, weighing eleven pounds, was solid, and had the appearance of liver throughout.

Dr. Peaslee said, Dr. Thomas's paper is a step in the right direction, and is full of suggestions.

It is said that the encephaloid form is the most frequent, and of larger size. Scirrhus does not attain to a large size.

Keith, in 105 cases of ovarian disease, has seen only three of cancer.

Spencer Wells, in 214 cases operated on, reports three cases of cancer; two of them were not supposed to be cancer at the time of the operation. He has specified two cases as adenoid, precisely as in Dr. Thomas's second case; one as large as a pint, both dendritic in appearance.

In the case where I removed both ovaries, the papilli pointed outward instead of within, as in Dr. Noeggerath's case, and was not malignant.

If the sac burst, you have the appearance of fungus hæmatodes. I have seen only four cases of cancer of the ovary; two of them were removed. Post-mortem examination was made in two cases.

I believe the œdema of the lower extremities, a cachectic appearance, and a large amount of ascitic fluid, are found in cases where the papilli are turned outward, for in such cases the fluid is inside the peritoneal cavity. When the fluid is inside of the cyst, the cyst is smaller, and its inside is studded with villi; these peculiar villi can only be detected by the microscope.

There are two kinds of cysts of the ovary, one glandular and one papular. The ovary is originally nothing else than mucous membrane, and either papular or glandular cysts may be developed in it.

W occasionally see a number of simple or benign cysts and cancer in the same way; both proceed from original structures. Graafian vesicles may be developed either into ovarian cysts or cancer; both take place in the same tissue, and that explains why we have them in the ovary, and why it is so difficult to make a diagnosis.

The Academy then adjourned.

Stated Meeting, March 22, 1871. Dr. E. R. Peaslee, President, in the Chair.

The President exhibited some micro-photographs of different objects, prepared by Dr. R. M. Fuller. One of them showed crystals of arsenious acid, from a part of a drop of fluid taken from the stomach of a woman supposed to have been poisoned.

The President also exhibited an instrument to facilitate the application of remedies to the cavity of the uterus, the same as he exhibited some time since, with a staff attached, to facilitate its introduction into the canal of the cervix.

Dr. Howard exhibited a patient, Mr. ———, aged 30, who was wounded by a shell at the battle of Gettysburg, having had a portion of the calf of right leg destroyed. The wound would not heal entirely, but remained in an indolent condition. Three weeks ago the wound was about six inches long and one inch wide, and had remained about in that condition for eight years, in spite of all the various kinds of treatment to which it had been subjected. At that time Dr. Howard removed two pieces of in-

tegument, about the size of a grain of rice, from the abdomen, and inserted them in grooves made for them on the surface of the ulcer. They were secured in their new places by skin-plaster, which was removed at the end of four days, when the pieces of skin were found to be adherent. He then inserted two pieces more, which became adherent in two days.

The entire surface of the margin of the ulcer seemed to commence healing at once, more rapidly at a point opposite the pieces inserted nearest the margin of the ulcer. The ulcer is now almost entirely healed. Dr. Howard prefers a sharp knife to scissors for removing pieces of integument to be transplanted, as it produces less laceration of the vessels.

Dr. F. H. Hamilton was the first surgeon who performed this operation in this city, at Charity Hospital, in July last.

Dr. Jenkins reported four successful cases of transplanting by Dr. J. H. Pooley, of Yonkers.

Dr. J. W. Blake, by invitation, exhibited a case of grape-shot wound of the chest. Col. ——— was wounded at the battle of Port Hudson by a grape-shot, $1\frac{1}{4}$ inch in diameter, which entered the chest just over the right clavicle, passed downwards and inwards through the lung, and lodged beneath the skin near the sixth dorsal vertebra. The first rib was fractured. Air passed in and out of the lung through the wound. At first he had a great deal of dyspnœa, and hæmoptysis occurred several times. His right arm was paralyzed for a long time, but at present he has free use of it. He was sent North, and was under the care of different surgeons. At the time he came under Dr. Blake's care he was very much emaciated, had a severe cough, hæmoptysis, etc., and was apparently in the last stage of phthisis. As there was a very free discharge from the wound, Dr. Blake concluded that there must be some foreign body in it. On making a careful examination he discovered and removed, with a good deal of difficulty, a piece of bone about two inches in length, probably a piece of the first rib. After its removal the wound gradually healed, and he rapidly regained his health and strength, and is now a strong, robust man.

Stated Meeting, April 6, 1871. Dr. E. R. Peaslee, President, in the Chair.

Drs. Cornelius E. Billington, Robert M. Fuller, Morris H. Henry, and Alfred E. M. Purdy were elected Resident Fellows.

Dr. S. F. Spier then read a paper on "The Use of the Microscope in the Differential Diagnosis of Morbid Growths, with a new Method for Determining the Diagnosis, Prognosis, and Treatment of Tumors and Cancers."

Dr. Krackowizer said, the paper is one of much original merit, though it fails in not taking sufficient notice of the doings of others in the same line of research. He ought to try and examine the merits of others.

By retaining the term cancer the paper may bring us back again to confusion.

Giving the name cancer to all malignant growths seems to be receding.

It is true that all growths are malignant as far as they spread and infect the whole system.

No cell originates unless it comes from another cell.

Those of extensive examination have decided to abandon the use of the term cancer, or to take one group of cells and call it cancer, and not apply it to any other group of cells.

Billroth says that all species of malignant growth have a difference of character.

Cancer is that form of malignant disease marked by epithelium cells.

It cannot originate in osseous and fatty structures. Some organs seem to be exempt, others are the selected seats of the disease.

It has never originated in the mucous membranes, skin, bones, or lymphatic glands. True metastasis cells are not fixed bodies, but retain an original power of locomotion, and have the power to wander and reach lymphathic glands which become affected. These cells find their way into the blood, and then go to all parts of the body. The theory of Dr. Spier is retrogressive, and plunges us back into confusion, and is not in consonance with the views of those who have paid special attention to the subject.

The Academy then adjourned.

Stated Meeting, April 20, 1871. Dr. E. R. Peaslee, President, in the Chair.

Drs. Thomas K. Chambers and William H. Dickinson, of London; Carl Rokitanski, of Vienna; Rudolph Virchow, of Berlin; and Felix Nélaton, of Paris, were elected Corresponding Fellows.

Drs. George B. Wood and Alfred Stillé, of Philadelphia, and Henry J. Bowditch, of Boston, were elected Honorary Fellows.

Dr. Benjamin Howard presented a case of primary resection of the humerus.

The patient, aged 30, received a gunshot fracture of the shaft of the right humerus, at about the union of the middle with the lower third, at the battle of the Wilderness. In order to secure apposition and rest, Dr. Howard made an incision on the posterior surface of the arm, turned the ends outwards, and sawed them off obliquely, removing about one and a half inch in all.

Over the anterior and posterior surfaces of each fragment he drilled holes and inserted annealed iron wire sutures, and brought them together. He closed the wound with silver wire sutures, and placed the arm in a bark splint. The patient was then removed to Washington, where he remained in hospital three months, during which time one of the iron wire sutures, with a small piece of necrosed bone, came away. It is not known what became of the other iron wire suture. The patient is well, and has free use of his arm, though it is about one and a half inch shorter than the other. Dr. Howard said that the operation is not one adapted to the field of battle, and that he could not recommend it in private practice. In this case he endeavored to preserve the periosteum as much as possible. In a case where a piece of shell had lodged in the femur, and caused a fracture a few inches above the knee-joint, he introduced one iron wire suture with a good result.

Dr. J. G. Adams alluded to the importance of the new remedy, hydrate of chloral, and suggested that the members give their experience in regard to its use.

Dr. S. T. Hubbard said he had used it successfully in three

cases of delirium tremens where opium had failed. He had used it in a case of hysteria, in fifteen-grain doses every two hours, until sleep ensued.

Dr. Joel Foster stated that in a case of approaching delirium tremens he had ordered fifteen grains, to be repeated in three hours; but the patient, by mistake, took ninety grains at once, which caused sleep during the night, but in the morning he had an intermittent pulse and great prostration. He gave quinine and porter to revive him, and he recovered. He also inquired if any of the Fellows had known chloral to produce an intermitting pulse?

Dr. Jenkins said he had given it to a lady, an opium-eater, in doses of seven and a half grains, which produced sleep for twelve hours, and for some time after she awoke she had an intermitting pulse. After that five grains were sufficient to produce sleep. On increasing the dose to seven and a half again, the same effect was produced as by the first dose.

It has been claimed that it produces its bad effects only when given in syrup, as it is supposed the syrup decomposes it. When given in water it produces no bad effect.

Dr. Farnham stated that some of the chloral manufactured here had produced bad effects on account of its impurity.

Dr. Hunt said he had heard of a case in the country where death was produced in thirty minutes by a dose of twenty grains of chloral. Some of the same was administered to a cat, and caused death in a few minutes.

The President stated that he had used chloral almost daily from the time of its first introduction here, and with no bad result. In one case there was some coldness of the surface, sweating, etc. He gives it in doses of from ten to fifteen grains. In the case of a lady, an opium-eater, chloral was substituted for the opium, and now she takes at night thirty grains, which causes sleep. The opium has been reduced to one-fourth of the original quantity.

Dr. Van Kleek said he had seen it given to the amount of seventy grains in one day, in a case of delirium tremens, without producing any effect on the disease.

Dr. Adams mentioned a case of hay asthma in a medical

man, where a single dose of fifteen grains of chloral relieved all symptoms of the disease, and they did not return that season.

Dr. Caro said that he had used it in a case of strangulated hernia in an old man, where tobacco, belladonna, chloroform, etc., had failed. A dose of twenty grains produced such relaxation of the parts that the hernia was easily reduced, but at the same time the man was so much prostrated that he appeared to be dead. Injections of brandy and milk revived him, and he recovered. He has also used it for rigidity of the os uteri in labor, with good success.

Dr. Pooley, of Yonkers, said he had used it in a case of large and weak heart (in a lady), in doses of thirty grains every night for five months. It produces sleep and a good night's rest, and seems to have the same effect now as at first. Dr. P. is in the habit of giving chloral to persons of all ages, and to an adult in doses of thirty grains. He gave sixty-seven grains at a dose in a case of mania, which recovered. In a few cases he has noticed swelling and itching of the eyelids follow its use.

Dr. A. N. Bell reported a case convalescent from apoplexy, followed by paralysis of left eyelid and arm, where, after giving sixty grains of bromid. potass. in twenty minutes, he gave fifteen grains of chloral with good effect. He believes that chloral fills a gap in the use of potass. bromid., and is in the habit of prescribing it in small doses after the bromide has been given.

In delirium tremens, his rule is to give chloral in doses of fifteen grains every three hours until three doses have been given, and then to give bromide of potassium. He has never been unfortunate enough to notice any bad effects or intermittence of the pulse. In concluding, he remarked that if chloral acts at all, it will act quickly, and within twenty minutes from the time of the first dose.

Dr. J. C. Peters spoke of a case of enlargement of the heart which had been under his care, where hydrate of chloral had acted well. The angina pectoris had been removed by its use.

Dr. Van Kleek said that Dr. Squibb, an excellent authority, had announced that chloral became decomposed when suspended in syrup.

He did not believe that chloral was necessarily dangerous, even if chloroform was liberated in the blood, as had been con-

tended by some, for he had occasionally given chloroform internally, in ten to twenty drop doses, with good results.

Dr. A. C. Post said he was once called to a case of attempted suicide, where as much as a teaspoonful of chloroform had been taken internally. When he arrived the patient was livid and in a comatose state, but he afterwards rallied.

Dr. Post stated that the last time he had occasion to use chloral was in a case where the patient would groan and start in her sleep. Fifteen grains were given, and in two hours twenty grains more, and the paroxysms did not return during the latter part of the night.

The President alluded to a case of scarlet fever in a child, attended with spasms, who was kept under the influence of chloral for twelve days and nights, and finally recovered. It was given in three and a half grain doses.

Dr. A. C. Post reported the case of a child who had chorea after having been struck with a billet of wood on the occiput. There was no necrosis afterwards. He regarded this case as a unique one, and was not aware that there was a case on record where chorea followed a traumatic injury without necrosis.

The Academy then adjourned.

Stated Meeting, May 4, 1871. Dr. E. R. Peaslee, President, in the Chair.

The President announced that the subject for the evening was a discussion of Dr. Spier's paper on "Morbid Growths," read April 6, 1871.

Dr. Dalton said he was interested in the illustrations given in Dr. Spier's paper.

One of Dr. Krackowizer's objections is well founded, and when we say a tumor is heterologous it makes a sharp line of distinction between the two.

It is curious to see that these identical questions were discussed in the French Academy twenty years ago. Velpeau said that the microscope was useless, and that it was easier to distinguish a cancer by its growth, appearance, etc., than by the microscope.

During the last twenty years microscopical anatomy has made great advances.

The surgeon's view of cancer is a tumor that destroys, and returns if removed; that is the clinical idea. The microscopical anatomist is not satisfied with that, for when he knows what fatty or fibrous tumor is, he wants to know what the peculiar character of cancer is.

From our two different stand-points—

Microscopical anatomists are endeavoring to find out what anatomical element is peculiar to cancer. All kinds that at one time were called cancer were found out not to be so.

Epithelioma is not regarded as cancer when it begins in a follicle. When it is cut out it always returns in the same spot. They consist of fibro-plastic cells. Some of the most rapidly growing are composed of nuclei, and when extirpated return.

You do not find this form of tumor in internal organs. They do not affect the constitution, and if they do not return in the same spot, do not return at all. My own experience would lead me to place reliance on the views expressed in Dr. Spier's paper. The cell cannot always be described. The nucleus is always characteristic, oval larger size from 2,000 to 1,200 of an inch in diameter, nucleoli one-half the size of a blood-globule, or 8,000 of an inch in diameter. It seems to me that we have a cell element which is peculiar, the nucleus one-third to one-half the size of the nucleus of a fibro-plastic cell.

There is one, a natural growth of ganglionic nerve-cell, and I do not know how to distinguish between them, that is, whether it is a ganglionic nerve-cell or a nucleus.

There is a strong tendency to point out how one cell was developed out of another cell, how red blood-globules are developed out of white ones.

To say that one is developed out of the other is not true. The development can always be proved to be different.

Dr. Krackowizer said:

Every page, Mr. President, of the paper to which we listened four weeks ago, bears the stamp of a mind fully familiar with the use of the microscope, and largely conversant with the accepted theories of cellular physiology and pathology.

In it we are informed that a law has been detected making possible the classification of morbid growths.

If so, then a long delayed desire has been filled.

Knowing as I do the author as one of the most skilful and reliable microscopists of this country, and much inferior as I feel myself to be in this branch of medical science, I should never have undertaken to express an opinion on the memoir before us, had it not devolved on me by your direct summons, Mr. President. But I console and fortify myself with this, that one need not be a master to criticise the works of a master.

Before entering on the task, I will try to reproduce the leading ideas of the author as I understand them. If I have misunderstood him, it does not necessarily follow that he is right, although my arguments have been misdirected.

I understand the author to be in accord with all the modern writers in stating, that from the fructification of the germ-formation and growth are the result of multiplication of pre-existing cells, branching off in different forms and assuming different relations. In this way different systems of tissues, and by their combination organs are formed, which, being harmoniously arranged according to a certain type, constitute what we call the normal human frame.

Ignoring, as is his right, as not pertaining to the question at issue, the inflammatory process, he states that in such tissues and organs the physiological act of nutrition can become so misdirected, that cells are formed so abnormal in number, shape, and relations that they constitute what we call a morbid growth, a tumor.

I may here mention in parenthesis that the views just expressed did throw overboard the old ones of *heteroplasty* and *homoplasty* (Dupuytren), and of *euplasty* and *kakoplasty* (Lobstein). With these must not be confounded *heterology* and *homology*, terms expressly coined to state the modern view as proclaimed mainly by Virchow, and which our author adopts.

Heterology (page 12) he calls an error in the arrangement of the elements composing a tumor as *to place or situation; homology* as regards the *quantity* of elements in the structure.

But the author does not stop here; he enlarges the definition of heterologous and homologous in a sense which is original

with him in this: that he applies the terms heterology and homology to the condition of the elementary constituents of tumors, taken as not connected with their site, but with their individual, innermost structure.

To him in this sense *heterologous* is a tumor which contains cells and derivatives of cells of *different* types; *homologous*, a tumor in which all elements are of *one* type.

I think the author would have been easier understood by selecting for one kind the word "*compound*," and for the other the word "*uniform*."

The idea of organization of course includes the idea of difference; but differentiation and heterology, if not by etymology, certainly by usage, are not the same in our mode of thinking.

Still this, in my view, unhappy choice of words does not vitiate the author's theory of classification of tumors, which he divides into *malignant* and *benign*, better called *non-malignant.*

I perceive that the author uses the terms *malignant* and *cancer* as equivalents, and I will admit that from his stand-point there is no inconsistency in their vicarious use. He ignores indeed the fact that most of the best modern investigators have adopted the word "cancer, carcinoma" for a histogenetically and histologically very well-defined species of morbid growth. In my opinion such authors have made thereby a very long stride in clearing up and removing misunderstandings, and I deem it unfortunate that the author's researches have not, as it seems, made him familiar with the reasons why this school of pathologists have proposed this nomenclature.

But as he claims to have detected a higher, an unerring principle, by which to distinguish anatomically malignant from non-malignant growths, I accord to him the perfect right to ignore propositions from other sides. If he can maintain his ground, it is for him to establish the nomenclature of tumors, and for others to adopt it.

Making use of his thorough knowledge of embryology and of normal and pathological anatomy, in his propositions 4, 5, and 6, as well as in several other pages of his paper, the author lays down his leading ideas. They are not rarely somewhat obscured by ontological and teleological reveries, into which

even very strong intellects, possessing highly imaginative powers and a brilliant style of writing, are sometimes drawn.

But the following ideas are not to be mistaken:

Their essence is—

That the homogeneous, indifferent, embryoplastic cell is the only one from which all the varied cells of the perfect and staple structures are formed;

That in the production of morbid growths there is a departure from this standard, by the cells producing either more elaborate or more simple specimens in the scale of cell development;

That such cells acquire abnormal relations to each other or to the parts of the body in which they germinate, or to both;

That the more different the individual cells in one morbid growth are to each other (heterologous in the new sense), and at the same time the more extraneous to the parts from which they develop (heterologous in the old sense), the more certain it is that the new growth is *not malignant* (page 15);

That with this condition of the cells of a tumor which as a whole is more germane to the seat from which it springs (homologous in the old sense), the new growth is *hardly ever malignant* (page 16);

That if all the cells composing a tumor are of one type (homologous in the new sense), yet if the tumor springs from a tissue or organ having a structure not related to the elementary structure of the tumor itself (heterologous in the old sense), it is *malignant* (pages 15 and 16);

That if the type of the cells composing a tumor is uniform (homologous in the new sense) and related to the tissue or organ of its growth (homologous in the old sense), it is certainly *malignant* (page 16);

That the uniformity (homology in the new sense) of the cells composing a tumor is the greater, the more the cells in the descending scale from the staple structures approach or coincide with the embryoplastic cell, and that such growths are never otherwise than malignant;

That therefore the presence of the embryoplastic cell in a tumor stamps it with the indelible brand of malignity;

That what authors describe as free nuclei in morbid growths are indeed embryoplastic cells (page 11);

That from these embryoplastic cells of a tumor, to which cells of a higher type of a once normal tissue or organ have been degraded, are developed in the tumor itself, and at a subsequent period, the " simple cancer cell " and the " compound cancer cell " (mother cell);

That this latter, although an unerring sign of the cancerous nature of a tumor, does not make it more malignant than the embryoplastic cell; that indeed its value in the estimation of the nature of a tumor is only so highly priced, because it speaks even to those unmistakably who are not so very well versed in the use of the microscope.

I believe these are the main theses of the author's theory.

They are mere assertions, either fully erroneous or only partially true, if we should find that they are not in consonance with the facts.

To begin with.

I hardly know that any such growths exist for which the author vindicates the quality of homogeneity (in his sense) except enchondroma and cholesteatoma (of Johann Mueller, Virchow's Perlgeschwulst, pearly tumor). I think these two growths of all alone come up to the point of what he calls " homologous to its own cells." In enchondroma (and more particularly in its hyaline variety) there are none but cartilage cells in a structureless intercellular substance; in cholesteatoma there are only epithelial cells arranged in globes, embedded in alveoli composed of epithelial cells too, with no intercellular substance whatsoever.

All the other tumors, in the author's own sense, are heterologous as to their own elements. Even his encephaloid tumor (which, I take it, comprises soft carcinoma and the round cell-sarcoma of the German authors) have their cells (very uniform in type, indeed, and in the sarcoma representing the most indifferent connective tissue cell) embedded in a more or less marked stroma of fibrillous or structureless connective tissue; they are richly provided with arteries, veins, lymphatics, perhaps with nerves, forming, in fact, a very complicated tissue—very heterologous, in the author's sense.

Or does he claim that such adventitious elements as connective tissue, vessels, and so on, must be excluded from the con-

sideration as accidental, when there is no doubt that they are not merely remnants of the tissues once healthy in which the tumor developed, but that they are new formations, built up *pari passu* with the cells?

But were the author to choose this ground, he could not do it without weakening his position in another direction.

Exclude connective tissue, vessels, nerves, as not understood to be counted in when the question of heterology or homology, in both senses, is to be decided, is there anything more "homologous as to its own cells and to the mother tissue," than *lipoma*, originating in the adipose tissue and composed of fat-cells? Yet where is its A 1 malignity?

The same with *exostosis*, *myoma* of the womb.

Again, take simple *hyperplasia* of the lymphatic glands, *leucœmic enlargement* of the same (certainly not malignant in the author's sense), and *lymphoma* (hardly outranked for malignity, in a clinical sense, by any tumor), yet all three are homologous as to their own cells, and homologous to the tissue which is their seat.

Take for example *enchondroma.* It is certainly homologous "as to its own cells." You find it in the parotid gland, certainly heterologous "as to the other tissues." When do you find it malignant just in this region, while not very rarely, developing in bony structures, it becomes malignant?

These examples might be multiplied, and the inconsistency and fallacy of the theory made more apparent.

In the examples just cited the histological character of tumors alone is considered. But the erroneousness of the author's theory becomes even more prominent when we criticise it from the *histogenetic* stand-point.

The author's paper, in several parts, gives evidence that he has not paid that very strict attention to the histogenesis of tumors which is so indispensable when the discussion refers to the primary starting-point of tumors.

For example, in stating the varieties of *epithelioma* (page 21), he claims "it is benign as long as there is any heterology of structure as concerns the cellular elements." Now epithelioma, in its very first commencement, is always developed either from the rete Malpighii alone, or from the epithelium of

the follicles of the skin, or from both, or from the epithelium of the mucous membrane or its glandular derivations. In this stage even it is never composed of anything but epithelial cells. Of course, as long as these single foci (which at first perhaps cannot be distinguished from an enlarged follicle) have not by depositing new morbid mass met each other, and have not coalesced into a uniform substance at the expense of the interstitial connective tissue of the skin, we may and must find elements of this; but not as a histogenetically integral part of the epithelioma, but as a remnant of once healthy tissue, stimulated, may be, to a little more active development of connective tissue cells by the approach of the multiplying epithelial cells. Each focus, originating from a follicle, is a separate epithelioma, bound to grow, and by encountering its mates to form a larger tumor, which will never stop growing until it has destroyed the patient, either by its local action, or by this and metastatic deposits combined. It has its nature from the beginning, having only one result. It is as malignant (clinically speaking) in the first focus (perhaps not perceptible by the common means of investigation), yet hidden in the skin, as after it has invaded and transformed muscles and bones and inner organs.

So it is with carcinoma, in the acceptation of the word by the German pathologists.

In one other point I differ with the author as to a fact. I do not think that the "simple cancer-cell," as described and delineated by him, is the return of a higher type of cells to the original type of the embryoplastic cell. His drawing makes to me the most decided impression that they *are* drawings—free nuclei, indeed, never occurring free though in the tumor while yet connected with the living body, but a mere archefact, through the specimen losing its freshness, or by the use of an unsuitable menstruum under the microscope.

More than others, the author's encephaloid tumors (Virchow's round cell sarcoma) have cells of which the membrane is exceedingly transparent, and so very tender that in the water under the microscope it quickly disappears. In this way the impression originates as if tumors were composed of free nuclei.

The author's theory in that part in which it is original is in-

consistent, full of contradiction, and it does not advance us a single step in the better knowledge of morbid growths.

It could result in nothing better, starting as it did from an entirely wrong principle.

The extensive knowledge and the great experience of the author have been misdirected in the futile attempt to call on the microscope for the solution of a problem, to do which the microscope should never be appealed to, because it cannot do it.

The question of malignity or non-malignity is one of clinical estimation entirely.

I would not that the student of morbid anatomy should ignore it; but not because he is a student of morbid anatomy, but because no physician ought to be a student of morbid anatomy *only.*

In the practice of medicine as an art, even empiricism is perfectly legitimate where science does not furnish any light. But at least so much we must do, that before a rationale has been found, we have ever to keep before us that our mode of explanation, consequently even our designations, are purely provisional. In this manner many very different objects may be grouped together with practical profit, only in their relation to a certain purpose to be attained.

But in the cultivation of medicine as a *science* it is not permitted to depart from the method inexorably required by the study of physical and natural sciences, of which medicine only forms a branch.

When we see that with ever so many well-observed facts the way of *induction* rarely permits us more than to find the laws which govern the occurrences, but hardly ever to detect even the proximate causes; when we reflect that even when it seems almost beyond doubt that we have detected a cause, it is quite the exception that in the corroborative and controlling way of *deduction* we can develop the phenomenon from its cause; I think we ought to be very careful not to attempt too soon to establish theories from incomplete premises.

The ideas of malignity and non-malignity are transmitted to us from a time when of the more intimate structure of morbid growths, and the manner in which they originate, nothing was

known; from a time in which the clinical standpoint was supreme in theory as well as in practice.

Pathologists have respected the latter claim; but in investigating the structure and natural history of morbid growths they became aware, not merely that they had to learn everything from the foundation, but that they had to clear away a good deal of rubbish of previous times, merely to come to an understanding among themselves what it was they wanted to accomplish by common work.

Of all this rubbish, the old ideas of malignity and non-malignity still exert the most baleful dominion. They are entirely teleological and inadmissible; they are to be cast aside mercilessly when it comes to writing the natural history of morbid growths.

They are subordinate, one-sided ideas, and therefore not fit to establish the supreme principle by which to classify tumors.

They are utilitarian, and as such foreign to the study of natural science.

We would deride botanists if they attempted to classify the vegetable kingdom in three large groups—nutritious, indifferent, and poisonous plants; we would scoff at zoologists if they classified the animal kingdom into domestic and wild animals. But without hesitation we waste our precious time and our best efforts to find out by the microscope whether a tumor is malignant or non-malignant, when all that is required and possible at present is to decide how it is constituted, how it originates, how it exists, and how it differs from others.

With all the progress we have made in the study of tumors, what we have yet to learn will be better and quicker reached by tracing *histological differences*, than by hunting after some *common sign*—call it cancer-cell or otherwise.

If no more division is possible, then what is common will be apparent, and the relative values of the common qualities will range themselves in natural orders.

It may be that *then* a never-wanting element of the minute structure or a peculiar mode of development will be found, on which it depends that certain tumors destroy locally, that they reappear after removal, and that they give rise to similar growths in other parts of the body.

But so far we can only say that no specific cell or derivative of cell is the carrier or representative of the peculiarities just mentioned of some tumors.

And yet the field as at present cultivated is full of promise. That he select this field rather than that of barren teleological speculation, I would feel happy to see the author bend his great talents and his great proficiency in microscopic investigation.

It is worthy of his notice to study and to criticise, to refute if he can, why such eminent students as Virchow, Tiersch, Billroth, Luecke, Waldeyer, Recklinghausen, Klebs, Rindfleisch, Koester, and many more retain the name "cancer" for a certain group of tumors, for which they vindicate an unmistakably epithelial character.

It is worth his while to investigate whether epithelial cells of morbid growths are produced by pre-existing epithelial cells only, or whether connective tissue cells can assume that peculiar atactic energy to produce them;

Whether the well-known difference of the three layers of the germinative membrane of the ovum remains persistent through life (His, Aeby, Tiersch), in this that the inner and outer layers can only produce epithelial (epidermic) structure in the way of repair of daily waste or in the way of abnormal growth, whereas from the middle layer are derived the connective tissue, with the systems and organs derived from its transmutations, and *vice versâ* all the morbid growths which have no epithelial character;

Whether in epithelioma and carcinoma the first departure from the norm is seen in the rete Malpighii, the follicles of the skin, the epithelium of the mucous membrane, its own glands and the other large glands which are derivatives of the inner layer of the germinative membrane, as Tiersch and others maintain; or whether the endothelium of the lymphatics is the primary seat of that morbid production of cells that finally constitutes cancer, as Koester has it;

Whether the newly-detected and well-studied quality of some cells to migrate (first hinted at by Virchow, then demonstrated by Recklinghausen, Cohnheim, and others) is or is not the cause why tumors that seemingly are completely eradicated are followed by a reappearance near the field of operation;

Whether, when such cells or even larger particles of neoplasms (emboli) get into the lymphatics or veins, and when they are arrested in other parts of the circulatory system, they are able to retain their vitality, and consequently the ability to multiply and produce growths identical with the mother tissue.

These are the questions that at present occupy the best observers and thinkers enlisted in furthering the knowledge about morbid growths.

That the author may range himself with them, and help to push the common work, is my sincere wish.

Solitary labor seldom builds up something lasting; co-operation should not be eschewed by the best talents and by the most original minds, if the danger would be avoided of being led astray.

I have extended my remarks far beyond what I originally intended, and yet they are only crude and fragmentary. I must plead the extensiveness of the subject for an excuse, and more even the consideration that it would require one far better informed and far more acute in reasoning to bring out the salient points in the author's memoir, as well as in the doctrine of tumors as it stands at present.

Dr. John C. Peters said:

Mr. President and Fellows of the Academy:—I had scarcely expected to be called upon to make any remarks this evening. We have been highly interested, and if all of us have not been instructed by Dr. Spier's extended paper, it is perhaps because the subject is so vast, and so many differing and even conflicting topics are contained in it, that it is rather difficult to grasp the whole, or any part of it.

Leaving the new cell doctrine where Drs. Krackowizer, Dalton, and others have dropped it, and with the simple remark that we prefer their opinions and those of Billroth, Stricker, Moxon, and others, to those of the author, I turn to what is to me a more congenial part of the subject, viz., the medical and constitutional treatment of cancer, especially the more destructive, or so-called malignant forms.

It is comparatively easy for the surgeon to remove benign tumors, even if they are recurrent; but it is very different with

the destructive or malignant varieties. Almost every case of the latter kind which has occurred in my practice has been placed in the hands of the best surgeons that I could possibly think of, and the result has been speedily fatal in every instance; I am very certain that life has been shortened in every instance, and suffering has been intensified. In my present mood and experience I would not think of advising, or even permitting an operation for the removal of a recurrent-destructive, or so-called malignant tumor. I now place all my hopes upon constitutional and local medication, and it is full time that we should have some definitely settled principles for our guidance beyond those of mere empiricism, or even ordinary experience.

Dr. Spier prefers a combination of iodide of potash with belladonna as a liniment, by the persistent use of which he has dissipated growths which at first appeared only capable of removal by operation. Also iodide of potash with phosphate of ammonia internally, when used freely, is said by him to have a great effect on morbid growths; also the chlorate of potash, cod-liver oil, iron, belladonna, and arsenic. We have no rules or principles laid down for the selection of one of these remedies in preference to the other; but as we know that fatty degeneration has been followed hy disintegration and absorption of abnormal cellular parts, we may use cod-liver oil when we wish to produce this effect.

Again, tonics and nutrients may convert disordered cellular development into a normal cell growth, and iron may come in play.

Finally, we are said to have remedies which are specifically antagonistic to the morbid growths, such as belladonna, arsenic, chlorate of potash, etc.

Thus, in a very slight and imperfect way, we may bring the experience and suggestions of Dr. Spier within the domain of the principles and practice of medicine. But Thomas Weeden Cooke, of the London Cancer Hospital, has a much larger experience, and also has theoretical views of his own. He tells us that the revelations of the microscope have shown that the ultimate structure of tumors is not a heterologous or heterogeneous deposit altogether different from the healthy tissues; but a homogeneous or homologous, although imperfect develop-

ment, formed of the same blood and cells which sustain and form the whole body, and repair the waste which always accompanies our mortal existence. Hence all our remedial efforts should be directed to get at the back of the tumor, as it were, by so improving the quality of the blood that it can no longer create imperfectly developed cells.

Arsenic is not such a remedy; Cooke has tested it carefully in every variety of temperament and constitution, and has utterly failed in obtaining any benefit from it. The iodide of arsenic is also unsatisfactory.

Iron has the great advantage of not being injurious like arsenic, and is a valuable tonic in those numerous cases of cancer which are accompanied with anæmia. Cooke prefers the tincture of the sesquichloride combined with dilute phosphoric acid and glycerine or syrup.

Cooke has given conium largely, both as a vaunted specific and as a sedative. We think it not only useless, but more apt to produce stomach derangement and headache than any other anodyne.

We have used iodine much and hopefully in the shape of iodides of potassium, iron, and arsenic; but have not obtained any satisfactory results, at least not in scirrhus, in which disease the absorbent action is almost if not altogether in abeyance. Iodine may also prove injurious, for whatever greatly increases the absorbent power diminishes the vital power; and the cessation of the cancerous growth, according to Cooke, depends upon the creation of a pabulum so highly organized that it can no longer supply abortive and ill-nourished cells. The absorbent power of iodine may be exercised, not upon the tumor itself, but upon the newly formed healthy structures, and thus do mischief by intercepting those supplies of genuine nutriment which should go to form normal tissue, rather than the eccentric development termed cancer.

The alkalies, ammonia and soda, have been largely employed in cancer, but Cooke believes that the results obtained are only those produced by improvement of the digestive organs.

Many herbs have enjoyed repute as remedies in cancer, but few have received such general approval as the *Galium Assarine* or clover grass. Cooke says there is much honest testimony

in favor of this herb, not only from intelligent patients, but from medical men of excellent repute, such as Paget and others. Cooke has had such excellent accounts of it from patients who have been advised to use it, that he is constrained to think some real benefit may be obtained from it. Ladies have taken it under his superintendence for years, and the cancer has remained in abeyance. We think it may have some specific effects on the blood, so that under its influence the tendency to the formation of embryotic or abortive cells is diminished, and a check is given to the further development of vicious material.

The preparations of gold and manganese are not satisfactory.

The mineral acids have obtained less favorable mention than they deserve; Cooke says they are of the greatest benefit, and claim our highest approval. There are few cases of cancer which will not be benefited by the administration of such a tonic as these agents afford. The phosphoric and the hydrochloric acids are the most useful, and in combination with the compound tincture of bark, serpentary, columba, or orange-peel, or with some good preparation of iron, such as the sesquichloride or the phosphate, these acids may be continued for a long period, and afford the most remarkable results.

Finally, Cooke says there is only one other medicine which has a large empirical claim upon our attention. In his hands *cod-liver oil*, administered in the occult stage of scirrhus, has more nearly approached the character of a specific than any other agent. It seems to supply that aliment to the cells of new formation for want of which they droop from their rotund form, and lose the power of creating normal tissues. It is best given in a solution of hydrochloric acid, or in combination with the muriate tincture of iron. We thus, he thinks, have two of our greatest remedies acting in concert, and while affording support to each other, they give to the alimentary power a stimulus which leads to the formation of normal tissue in place of an irregular diseased growth.

Such is the latest and perhaps the best experience in the medical treatment of cancer. But there are many deficiencies and imperfections in it still, although some of these may be immediately removed by the latest microscopical discoveries.

Thus, since the discoveries of Cohnheim and others we know that the so-called plasma of the blood and the lymphoid cells often pass through the walls of relaxed and dilated capillaries. We are also perforce obliged to admit that spindle cells, connective tissue, corpuscles, and some others may be formed by simple modifications of the normal lymphoid blood-cells. Be this as it may, the microscope teaches us that both fluids and cells pass through the abnormally thinned or dilated walls of the capillaries, and tend to the growth of morbid as well as normal cells and tissues. Hence, in limiting the growth of cancers and tumors, we may, first of all, be obliged to compress or constringe the capillaries, and thus limit the profuse outpouring of fluids and cells into the morbid tissues and growths. The mineral acids, such as the muriate tincture of iron, are well in place here, and theory agrees with experience and practice. Quinine also prevents the migration or wandering out of white blood-cells through the capillaries, and may be profitably associated with the mineral acids.

Among all the sedative remedies the cannabis indica checks hemorrhages, and prevents effusions and exudations, more than any other, while it improves the appetite and digestion. A combination of it with iron or quinine is often exceedingly comforting, and partially curative.

But in all fungoid growths, and rapidly growing, and even medullary tumors, we may finally be obliged to resort to the most heroic and active constringents of the capillaries, viz., ergot. I have seen several fungoid growths of such size, rapid growth, and apparently destructive tendency, that they have been pronounced cancers by competent, cautious, and experienced physicians and surgeons, disappear entirely under its bold but cautious use. It may have to be combined with iron or quinine in reduced and delicate patients.

Among the local applications Cooke prefers the liquor plumbi, and I would not hesitate in appropriate cases to give the acetates of lead and iron internally.

Of all the local anæsthetics Cooke prefers belladonna, two drachms of the extract to six drachms of ceratum saponis, or three grains of atropine to a half or whole ounce of zinc ointment, as a cleaner and equally efficacious application. It re-

mains to be determined whether it is more useful than the local application of cannabis indica.

Cooke has seen ulcerated scirrhus heal under the influence of a lotion made of eight grains of chlorate of potash and two minims of strong hydrochloric acid in once ounce of distilled water. The chlorate of potash is also a better deodorizer than the permanganate of potash, or carbolic acid; for the odor of the latter is only less offensive than that which it is meant to correct; and Cooke says it has no lasting effect even in disguising it.

Thus I have endeavored to combine principles with experience, theory with practice, in the hope that the remedies that we know to be useful may be applied more wisely and carefully, as well as more persistently and thoroughly. At any rate, we have enough to rely upon which will justify us in encouraging our cancer patients to remain under the charge of experienced and capable medical men, instead of resorting to the votaries of crude empiricism or downright quackery.

I may add that I do not share Cooke's fears about the injurious effects of the preparations of potash; we know what enormous doses of the iodide are not only tolerated but required in syphilitic nodes and tumors, and how goître will melt away under the use of iodine; and although these remedies will not remove true cancer, still they will carry away much new and unhealthy deposits, thus lessening the size of the swelling. And I have certainly seen a double ovarian tumor, and a large one of the uterus, entirely disappear under the long-continued use of the bromide of potash and cannabis indica. It is well to add that the bromide checks hemorrhages and exudations, and contracts the capillaries far more decidedly than the iodide.

Dr. A. N. Bell said that he had read Dr. Spier's paper, and no one can doubt that it is a valuable contribution to the history of cancer. The differences of tumors are owing to the pabulum on which they feed. They have a physiology of their own, and undergo changes under different circumstances, whether they are hereditary or have started suddenly in certain individuals. As an attempt at classifying this subject, this paper is a valuable one. The paper is a step in advance, and differs from the systematic treatises on that subject.

Dr. Burrall said, as regards the treatment of cancer, remedies

have practically been used empirically. It should be our effort to discover some plan by which the cell growth may be retarded or checked. We may place tonics among the remedies that may be of use. I think we have certain agents which retard its growth. They may be arranged as tonics and antiseptics. Arsenic, tr. ferri chloridi, quinine, and carbolic acid may be used with benefit.

In one case I used quinine and carbolic acid internally, and carbolic acid locally, and the ulcer of the breast healed. Subsequently there was a solution of continuity which has never healed, though she is in fine health. In another case, an ulcer left after the removal of a scirrhus of breast healed under the use of quin. sulph. and acid-carbolic internally, and carbolic acid locally. Persulphate of iron applied to the ulcer changes its face.

In those cases that will not submit to an operation we should endeavor to alleviate the pain by local applications.

Dr. Chas. A. Hart then alluded to the value of Dr. Spier's artery constrictor in two recent cases. In one case of amputation of the leg the instrument was successfully applied to the two tibial arteries at the junction of the middle and lower thirds of the leg, without the loss of one drop of blood. Portions of the tibial and peroneal arteries were exhibited in a complete state of calcification. Date of application, March 9th.

Also in the case of a male infant of thirteen months, with a pulsating nævus, the size of an orange, at the angle of the jaw, the constrictor was applied to the external carotid artery at the middle of the neck. The wound united partly by first intention; since the operation the tumor has decreased in size about two-thirds, and no return of pulsation. Date of operation, March 11th.

STATED MEETING, MAY 18, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

Drs. Samuel Blume, Morris H. Henry, William B. Neftel, Alfred E. M. Purdy, Cornelius E. Billington, and Theodore Walser were inaugurated Resident Fellows.

NOTE.—In the report of the result of the Annual Election of the Academy, Jan. 5, 1871, the name of Dr. Alf. Underhill, as *Trustee*, was inadvertently omitted.—(COM. ON PUB.)

Dr. Charles P. Russell was elected a Resident Fellow.

Dr. Truehart, of Galveston, Texas, by invitation, exhibited some instruments recently brought by him from Germany after plans of his own devising: 1st. An ingenious instrument, mostly of glass, for the transfusion of blood. 2d. An apparatus for producing artificial respiration, the motive power being the lungs of the operator. It can also be used as a stomach pump. 3d. A trephine. 4th. A cranioclast. 5th. The 365th modification of the obstetrical forceps. By a peculiar construction of its lock, the blades are allowed to rotate a little, and are readily locked. The handles are covered with hard rubber.

Letters were read, by the Corresponding Secretary, from Drs. Wood, of Philadelphia, and Bowditch, of Boston, thanking the Academy for the honor of membership conferred on them, and the receipt of several books acknowledged.

Dr. Gurdon Buck then read a paper on the "Treatment of Cicatricial Contractions after Burns of the Face," and exhibited a patient, a boy about four years old, on whom he had made the operation described in his paper, in November last. The face had been severely burned, when about two years old, about the lower lip, chin, neck and breast, and contraction had brought the chin down near the sternum and left side. Four ingenious operations were performed under ether, and a brace applied to keep up a constant outward contraction; a stiff padded leather stock, or band, was attached to the upper portion of the contrivance, graduated by means of a screw, which compelled the head to be raised and allowed free play of the chin, which had been worn constantly, except at night. The results of the operations were highly satisfactory. Collodion was applied to promote an artificial scab.

Dr. J. C. Peters said that the patient had been first attended by him, and he had applied an instrument soon after the injury. It would have been better for the child, at first, if this instrument, which was thrown aside afterwards by the parents, contrarily to his advice, had been allowed to remain on.

After a few remarks from the President, Drs. Truehart and Hutchinson, the Academy adjourned.

STATED MEETING, JUNE 1, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

Drs. R. M. Fuller and W. H. B. Post were inaugurated Resident Fellows.

Drs. J. Ackerman Coles, Edward J. Hogan, Josiah C. Nott, and J. V. S. Woolley were elected Resident Fellows.

Dr. H. D. NOYES then read the paper for the evening, on "Paralysis of the Fifth Cerebral Nerve." Published elsewhere.

The Academy then adjourned.

STATED MEETING, JUNE 15, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

Dr. J. C. DALTON read the following paper on "Sugar Formation in the Liver."

The present condition of our knowledge on the glycogenic function is as follows:—

It is universally known that the liver in healthy animals, when examined within a few minutes after death, contains an appreciable amount of glucose; that this glucose increases in quantity in the liver tissue after the circulation has ceased; that it will even reappear in the liver, separated from the body, after having been entirely washed out by a continued watery injection of the hepatic vessels; and that it is produced by a catalytic transformation of the amyloid substance, or glycogene, under the influence of an animal ferment. All these facts, due to the remarkable discoveries of Bernard, have been abundantly confirmed by other experimenters, and are established in a manner which leaves no room for question.

It is doubted, however, at the present day, whether glucose really exists in the liver during life, and consequently whether there is any such thing as the glycogenic function, properly speaking. This doubt was first raised in 1858 by Pavy,* who asserted that the glucose found in the liver after death was a substance exclusively of post-mortem production; that the liv-

* Proceedings of the Royal Society of London, 1858. IX., p. 300.

ing organ contained only the amyloid substance, or "hepatine," as he called it, which, however, was transformed into glucose after death with extraordinary rapidity; but that the non-existence of sugar, as a physiological ingredient, could be proved by injecting the liver immediately after death with a solution of potash, or by instantly placing the portion cut off in a freezing mixture of ice and salt, either of which processes would arrest the catalytic transformation of the glycogene. This conclusion was entirely unwarranted by the experiments which Pavy reported, since in no single instance was glucose altogether absent from the liver tissue in healthy animals treated as above, or from the blood of the right ventricle, removed by catheterization during life. It was simply present in very minute quantity, as compared with that found in the organ after a short time had elapsed.

The opinion of Pavy was controverted by Harley in 1860,* who operated by killing the animals by section of the medulla oblongata, immediately placing a portion of the liver in the freezing mixture, and afterward slicing it directly into boiling acidulated water. He reported four of these experiments, in all of which the liver tissue, so treated, gave distinct evidence of sugar. In one of them, the time which elapsed from the death of the animal to the immersion of the liver tissue in the freezing mixture is given as "less than twenty seconds."

Other observers, however, adopted Pavy's view. Meissner,† in 1862, and Ritter,‡ in 1865, repeated the experiments in a slightly modified form, by suddenly slicing out a portion of the liver from the living rabbit, cutting it into small pieces, and immediately dropping it into boiling water. They found that the extract, prepared in this way, gave no reaction when examined by Trommer's test,—and conclude accordingly that the liver of the healthy animal, during life, contains no trace of glucose.

Schiff,§ in 1866, arrived at the same result in dogs, cats, rabbits, and guinea-pigs, by taking out a portion of liver at

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* Proceedings of the Royal Society of London. X., p. 289.

† Zeitschrift für rationelle Medicin. XIX., pp. 310, 311, 312.

‡ Zeitschrift für rationelle Medicin. XXIV., p. 65.

§ Journal de l'Anatomie et de la Physiologie. 3me année, No. 4, p. 354.

the instant of death, and cutting it up at once into boiling water.

On the other hand, Eulenberg,* in 1868, on repeating Ritter's experiments, found in every case traces of sugar, provided the copper test were thoroughly applied; and he concludes that the extract of the liver, prepared by the boiling water process, will always show evidence of glucose, if the test be employed with due care. He thinks, however, that the sugar so obtained may have been produced in the few instants of time required for cutting up the organ into small pieces,—and he accordingly adopted the plan of grinding the liver substance in a mortar with alcohol and pounded glass. The extracts which he obtained in this way, in six cases, were entirely destitute of sugar, when examined by the copper test.

Professor Flint, jr.,† in 1868, experimented upon dogs by cutting out a portion of the liver during life, slicing it into boiling water, and examining the extract by Trommer's and Fehling's tests. In two instances, where the time employed in the operation was respectively 28 seconds and 22 seconds, there was no marked or certain evidence of sugar. In one instance, where the time employed was only 10 seconds, the liver extract presented no trace of sugar whatever. The blood of the hepatic veins, however, obtained by ligature of the vena cava inferior within a minute after the first operation, showed a well-marked reaction with the copper test. Professor Flint concludes that the glycogenic matter is really converted into sugar by the liver during life, but is carried away from the organ, as fast as it is formed, by the current of the circulating blood.

Finally, Professor Lusk,‡ in 1870, operated upon five dogs, in order to determine the difference, if any, in the quantity of sugar contained in the blood of the right ventricle, removed by catheterization during life, and that of the jugular vein. He found in every instance, contrary to the results of Pavy, McDonnell, Meissner, and Ritter, that the sugar contained in the hepatic blood preponderated very considerably over that in the general circulation,—and that the quantity of glucose in the blood

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* Journal für praktische Chemie. CIII., p. 108.

† New York Medical Journal. January, 1869.

‡ New York Medical Journal. July, 1870.

of the right side of the heart was from two to four times greater than that found under the same circumstances in the jugular vein.

Two years ago, I was desirous of ascertaining the exact time within which glucose would fail to appear in the liver extract examined by the ordinary method. For this purpose I experimented upon dogs by cutting out a portion of the liver in the same manner as Professor Flint had done, slicing it into boiling water, and making an extract of the coagulated liver tissue by rubbing it to a pulp in a mortar, and treating different portions by boiling with pure water, boiling with an excess of sulphate of soda, and lixiviating with cold water through finely powdered animal charcoal. In one instance the liver substance, immediately on being removed from the body, was crushed between two slabs of ground glass, rubbed to a pasty mass with animal charcoal, and then lixiviated with cold water. The result was, that when the preliminary operations were completed in 17 seconds and 22 seconds, the final extract of the liver tissue gave no reduction by the copper test, but at the end of 50 seconds it gave rather slowly a distinct though not abundant indication of sugar. In one instance, different portions of the same liver were treated by boiling water, and afterward with animal charcoal, at the end of 17 seconds, and at the end of one, two, three, four, five, and seven minutes successively. In the first instance (17 seconds), there was no indication of sugar by Trommer's test; in the second (one minute), the sugar reaction was delicate but distinct. In the remaining five specimens the reaction, as appreciated by the eye, was constantly more and more marked. The appearance of the different test tubes, after the completion of the experiment, was very striking. In the first, representing the liver extract at the end of 17 seconds, the liquid remained perfectly blue and transparent; the remainder all showed a yellow or reddish color from the reduction of the copper, and varied only in the intensity of the hue and the quantity of deposit, which increased exactly in proportion to the time which had elapsed before the end of the operation.

According to these results, therefore, 50 seconds was the shortest time within which the liver tissue, removed from the living animal, could be found to give indication of the presence of sugar.

These experiments, however, were not fully satisfactory to me, for several reasons. In the first place, when a substance like glucose invariably appears in an animal tissue after death with such rapidity that the interval is to be counted by seconds, it naturally suggests the propriety of extreme caution in adopting the conclusion that it was not there before,—at least in minute quantity. Especially as nearly all observers are agreed that slight disturbances of the circulation or respiration, the struggles of the animal immediately before the operation, or the compressing effect of ligatures, will cause the appearance of glucose in the liver tissue at the instant of its removal, the necessity of such caution becomes very evident. Schiff states * that in various animals, by simply compressing the abdominal aorta for ten minutes, or tying the principal blood-vessels of one limb, he has produced a condition of diabetes; and, on killing the animal, has found sugar present in the liver, though examined with all the requisite precautions. Even the use of ether is interdicted in experiments of this nature, owing to its liability to bring on a saccharine condition of the liver during life. According to the original observations of Bernard, the glucose produced in the liver was supposed to be constantly carried away by the blood of the hepatic veins, to be replaced by a fresh supply, of new formation; so that a comparatively large amount of sugar might be supplied by the liver in twenty-four hours, and yet only a small quantity be present in the organ at any one time. We all know that, in point of fact, the amount of sugar in the liver tissue increases after stoppage of the circulation, just as urea accumulates in the blood after removal of the kidneys, or carbonic acid in the lungs after the stoppage of respiration. The question is, whether this increase of sugar is simply the accumulation of a substance already existing in small quantity, or a matter entirely of post-mortem production.

It must be remembered, furthermore, that the chemical tests for glucose, as well as for other substances, have their limits in point of delicacy; and it is possible that they may fail to detect its presence, in some instances, simply on account of its minute quantity. There was a time when it was impossible to detect

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* Journal de l'Anatomie et de la Physiologie. 3me année, No. 4, p. 365.

the presence of urea in healthy blood; and it was only after the requisite improvement in our chemical manipulations that this substance could be distinguished as a normal ingredient of the circulation. This consideration is of some importance in the present connection, because the quantity of liver tissue examined in the above experiments is of itself necessarily small. I have found it difficult to cut up in sufficiently thin pieces, and immerse in the boiling water within the requisite time, more than about 140 grains of the liver tissue. If a much larger quantity than this be used, it requires more time for completing the operation, and gives rise to the presumption that the sugar afterward found may have been produced by fermentation during the interval which has elapsed.

For these reasons I was anxious, in the first place, to determine the exact limits of sensibility of the various tests for sugar, and the best manner of employing them; and secondly, to contrive some plan by which a larger quantity of liver tissue might be used for experiment, without increasing the time consumed in the operation.

The most convenient and generally useful of all the means for detecting sugar is that known as Trommer's test. The imperfection in this test, however, is that it is indefinite in regard to quantity. The only rule commonly recognized for its application is to add to the suspected liquid, first, a solution of sulphate of copper, and then a solution of potassa, in sufficient quantity to give to the mixture a clear blue color and a distinctly alkaline reaction. On boiling, if glucose be present, the blue color changes to a light turbid yellow, or an opaque red tint, according to the purity of the liquids and the quantity of copper oxide precipitated.

This is quite sufficient for ordinary purposes. But with liquids containing only a minute quantity of sugar in a rather dilute form, if we add but very little sulphate of copper, the change of color on boiling may not be sufficiently marked to be satisfactory; and on the other hand, if we add a quantity of sulphate of copper large enough to make the mixture a decided blue, there is a similar difficulty from another cause. A definite quantity of sugar can only decompose a definite quantity of the

copper salt, and consequently the small amount of copper oxide precipitated may be masked by the blue color of that portion of the liquid remaining undecomposed. It is not easy, therefore, by this means, either to detect sugar in very small amount, or to estimate its absolute quantity in saccharine liquids.

It is claimed by the author of this test * that it will give a visible precipitate on boiling, in a liquid containing one part of grape sugar dissolved in 100,000 parts of water. I have not been able to succeed with it in liquids of so high a degree of dilution, though using every possible care. The most dilute solution in which I have found it yield an appreciable indication is a liquid containing one part of glucose to 10,000 parts of water; and even with this, in order to succeed, we must operate with at least 25 cubic centimetres of the solution—that is, a volume containing $\frac{1}{26}$ of a grain of sugar.

It is easier to detect the presence of sugar in small amount when it is examined in a more concentrated form. According to my experience, the smallest absolute quantity of glucose perceptible by Trommer's test is one cubic centimetre of a watery solution made in the proportion of one part to 2,000, and containing accordingly a little over $\frac{1}{130}$ of a grain of glucose.

Almen's bismuth solution † is far less delicate than the copper test. When freshly prepared the liquid is perfectly clear and colorless. On boiling with a saccharine solution, if glucose be present in decided quantity, it turns to a brownish hue, which on cooling rapidly changes to a nearly pure opaque black, very strongly marked when viewed against a white ground. With one cubic centimetre of a solution containing $\frac{1}{40}$ of a grain of glucose, the blackish color of the mixture after cooling is still

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* Bericht der Königl. Preuss. Akademie der Wissenschaften zu Berlin. 1841, p. 222.

† This solution is made as follows :—

Tartrate of soda and potassa, 160 grains, is dissolved in 4,000 grains of a solution of hydrate of potassa, having the specific gravity 1.33. To this mixture, when warmed, but not boiling, subnitrate of bismuth is added so long as it continues to be dissolved. After cooling, the clear liquor is decanted. The solution must not be made with the chemically pure potassa, which has been prepared with the aid of alcohol, but with the ordinary hydrate of potassa, in sticks.

distinctly perceptible, though it is not opaque; but with $\frac{1}{50}$ of a grain no characteristic reaction takes place.

The hope has sometimes been entertained that the rotation of the polarized ray by saccharine solutions would afford a more delicate test of the presence of sugar than that given by any chemical reaction. This hope, however, has not been realized. In order to produce a decided rotation, the polarized ray must pass through the saccharine solution for a considerable distance, usually about eight inches; and this requires a correspondingly large volume of the liquid under examination.

In Mitscherlich's apparatus, which was intended for medical purposes and is of comparatively simple construction, the tube is 20 centimetres in length, and requires, for application of the test, 28 cubic centimetres of the saccharine liquid. With a solution of cane sugar, made in the proportion of 30 parts to 100, the rotation is 36 degrees. With a solution of cane sugar, one part to 100, it is only from 1 to $2\frac{1}{2}$ degrees. With a solution of glucose (the rotatory power of which is less than that of cane sugar), if made in the proportion of one per cent., the rotation is from zero to 2 degrees. That is to say, with 28 cubic centimetres of such a solution, containing $4\frac{1}{3}$ grains of sugar, the glucose is practically inappreciable.

Soleil's saccharimeter is a much more elaborate and delicately constructed instrument. The tube is eight inches in length, and contains 13.75 cubic centimetres of liquid. The standard solution for this apparatus is a liquid containing 26.05 grammes of pure cane sugar dissolved in 100 cubic centimetres of water, —that is, with the sugar in the proportion of 23.68 parts per hundred. With this solution, the scale of the instrument should mark 100 degrees. In order to bring the index to this point, therefore, we must use 13.75 cubic centimetres of the standard solution, containing 55 grains of cane sugar.

Soleil's saccharimeter was intended for the use of sugar manufacturers, and is employed for testing the quantity of good sugar in a tolerably dense solution,—such as will mark somewhere about 30 degrees on the scale of the instrument. In France it is used, as I am informed by Professor Chandler, to test the quality of a raw sugar, by ascertaining its proportion of good sugar available for refining purposes, and in this way to fix its

price for the manufacturer. In this country, at least in some establishments, it is no longer used for that purpose, since the skilled purchaser finds that his inspection of the raw sugar by sight and touch is equally reliable. It is employed mostly for examination of the liquor drained from a crystallizing mass, in order to see how much uncrystallized sugar is still contained in it, and thus to avoid an undue loss in the manufacture.

Notwithstanding the improved construction of this instrument, with saccharine solutions of any grade of strength there is a range of one degree of the scale, in which there is no perceptible difference of color in the polarizing plates. Beside this, on each side of the above range there is a margin of at least one degree, in which the change of color is exceedingly faint, and requires the closest care and attention to be distinguished.

According to my experience, the weakest solution of cane sugar which can be distinguished by the polarizing test with any degree of certainty, is that of one part per thousand, and the necessary volume of such a solution contains $\frac{1}{5}$ of a grain of sugar.

On the other hand, a solution of glucose, made much weaker than one per cent., cannot be recognized with certainty; and a solution of one part per thousand gives no reliable indication whatever of the presence of glucose.

It is evident, therefore, that the polarizing apparatus cannot be relied upon for the detection of glucose in physiological investigations.

By far the most sensitive test for glucose yet discovered is that by Fehling's solution, which is a double tartrate of potash and copper, dissolved in an alkaline liquid, and containing in a given volume a definite quantity of the copper salt.* The extreme sensibility of this solution may be well shown by using it

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* Fehling's solution is made as follows :—

Pure crystallized sulphate of copper, 500 grains, is dissolved in about $4\frac{1}{2}$ fluid ounces of water.

Then, neutral tartrate of potassa, 2,000 grains, dissolved in a little water, is mixed with a solution of caustic soda (of the specific gravity 1.12), 8,750 grains.

To this alkaline liquid the copper solution is gradually added, the mixture taking a clear, deep blue color.

The whole is finally diluted with water to the volume of $934\frac{9}{10}$ cubic centimetres, or f. ℥ 31, f. ʒ 5.

in a dilute form. If mingled with 40 times its volume of water and using four cubic centimetres of the mixture, by adding a few drops of a saccharine solution containing $\frac{1}{160}$ of a grain of glucose, and bringing the whole to ebullition, the reaction which follows is brilliant when viewed against a black ground, and quite perceptible both against white and by transmitted light. If a mixture be made of Fehling's solution one part, and water 1,000 parts, by adding $\frac{1}{70}$ of a grain of glucose, there is still a distinct and perfectly characteristic reaction also visible in all lights, though seen to best advantage against a black ground.

The most effectual way of using this test, for very small quantities of glucose, is to make the following mixture:—

Fehling's solution....................	1 part.
Water....	2 parts.

Of this mixture, five cubic centimetres are placed in a narrow test tube, rather less than half an inch in diameter and about 3½ inches long. The tube should be placed in an oblique position, one inch in front of a background of black glass. It is fixed in this position by means of a cork collar, which embraces it at its upper extremity, and which is held by a metallic ring and screw, attached to a wooden framework behind.

The dilute copper solution is then raised to the boiling-point by the flame of a spirit lamp, care being taken not to apply the flame to the sides of the test tube above the level of the liquid. When the copper solution has thus been brought thoroughly to ebullition, the boiling is allowed to subside; and the saccharine liquid is then immediately added, drop by drop, from another test tube, in which it has already been kept hot for the purpose.

In this way the hot saccharine liquid, flowing down the inclined sides of the test tube, mingles gently with the surface layer of the copper solution; and when reaction takes place, it is indicated by a thin yellow or orange-colored ring at the surface of the mixture, which contrasts distinctly with the clear blue color of the remainder. Boiling must not be continued after the addition of the saccharine liquid;—for in that case the minute quantity of copper precipitate, which is perfectly distinct so long as it remains at rest, is broken up and diffused by

the mechanical agitation, and becomes quite imperceptible in the excess of the blue liquid.

The principal condition necessary for success, when testing by this method for sugar in minute quantity, is to have both liquids, at the moment of their admixture, as nearly as possible at the boiling-point without being disturbed by actual ebullition. The apparatus should be illuminated by clear white daylight, coming from a lateral direction. The black background is the best for showing a very delicate reaction; and in this way we can sometimes see distinctly a copper precipitate which would be distinguished with difficulty against a white ground, and quite imperceptible by ordinary transmitted light.

In every case there should be two test tubes, containing equal quantities of the copper solution, placed side by side in a similar position. Both the fluids are to be treated in the same manner, excepting that the saccharine solution is added only to one of them, the other being used simply for comparison, in order to secure accuracy in the results. By this means we avoid the danger of mistake from spontaneous decomposition of the test liquid.*

If Fehling's test be used in the manner now described, with a solution of glucose made in the proportion of one part per 10,000, three drops of the saccharine liquid, added to the hot mixture, will cause the appearance of a faint yellowish ring on the surface of the copper solution; but it is very delicate, and requires for its production every possible care in the manipulations.

With a solution of glucose made in the proportion of one grain to 100 cubic centimetres, one drop,† containing $\frac{1}{1000}$ of

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* Fehling's solution is apt to become changed in course of time, if freely exposed to the atmosphere, by a conversion of a portion of its tartaric into carbonic acid; after which it will partially precipitate on boiling, though no sugar be present. To guard against this, it is best to keep the solution in a number of small, well-stoppered bottles, each one of which, except that in actual use, is quite full of the liquid, and is thus protected from the action of the atmosphere. As soon as the solution in use shows signs of alteration, it is thrown away and a fresh bottle substituted. I have found Fehling's solution, when exposed to the air in warm weather, give indications of spontaneous decomposition at the end of a week; while portions of the same liquid, carefully kept in closed and full bottles, remained entirely unchanged for over three months.

† The drops used in these experiments were very nearly equal to one-tenth of a cubic centimetre each.

a grain of glucose, produces, in a few seconds, a slight but distinct reaction. If the same saccharine liquid be diluted with an equal volume of water, one drop, containing $\frac{1}{2000}$ of a grain of glucose, also produces a reaction ; but the reaction in this case is rather slow in making its appearance, and is on the extreme limit of certainty. On the other hand, $\frac{1}{3000}$ of a grain of glucose, added in a similar way, causes no recognizable reaction.

That is to say, $\frac{1}{2000}$ of a grain of glucose, *if concentrated in a single drop*, may be detected by Fehling's solution, used in this manner. At the same time, a smaller quantity than $\frac{1}{1000}$ of a grain is hardly available for practical purposes..

It would, however, be very inconvenient to concentrate the fluid extract of an animal tissue to so small a volume as one drop. The better way, on the whole, when we wish simply to determine the presence or absence of glucose in such cases, is to use about one cubic centimetre of the fluid extract and to add to it one drop of the pure Fehling's solution. This method is sufficiently delicate for all requisite examinations.

If one cubic centimetre of water, containing $\frac{1}{100}$ of a grain of glucose, be placed in a narrow test tube and one drop of Fehling's solution added, the reaction on boiling is prompt and very strong, easily visible in all lights and from a considerable distance.

With a similar quantity of water containing $\frac{1}{1000}$ of a grain of glucose, and treated as above, the reaction is a little tardy in its appearance, but is perfectly distinct in character, most marked when viewed against a black ground. With weaker solutions the reaction is less distinct, and soon becomes entirely imperceptible.

This accordingly is about the limit of the practical operation of Fehling's test. In delicate examinations the degree of concentration is always of some importance, since the same quantity of glucose, dissolved in double the quantity of water, will often fail to give a reaction, though easily detected in the more concentrated form.

For the purpose of reducing the liver tissue to a state of fine comminution in the shortest possible time, I employed a

machine of simple construction, but very effective in its operation, known as the "crimping machine." It consists of two fluted brass cylinders, placed horizontally side by side, and made to revolve rapidly in opposite directions by means of a crank handle. Each cylinder is six and a half inches in length and one and a half inch in diameter.

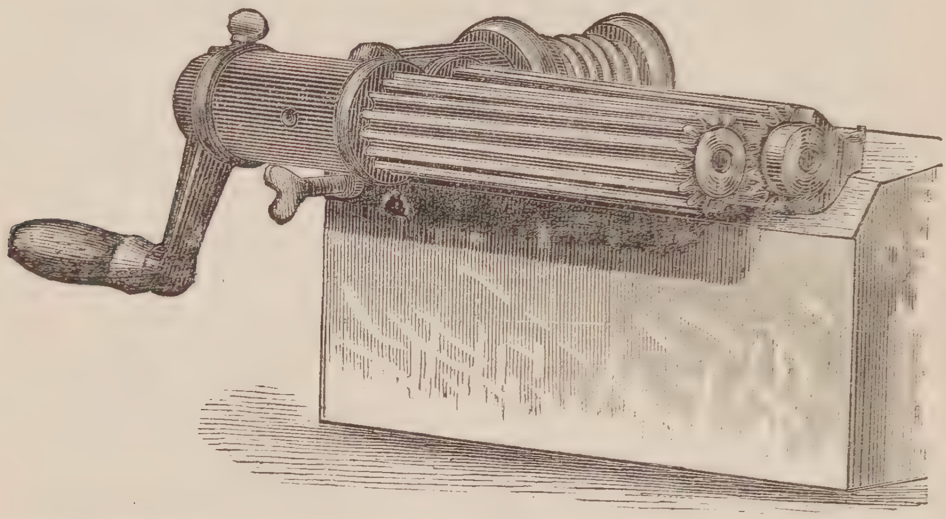

FIG. 1. MACHINE for comminuting the liver tissue ; about one-fifth the natural size.

During the revolution of the cylinders, their nearly parallel projections and depressions lock into each other like those of two cog-wheels ; their contiguous surfaces, at the point of greatest proximity, being separated by a distance of not more than one-sixteenth of an inch.

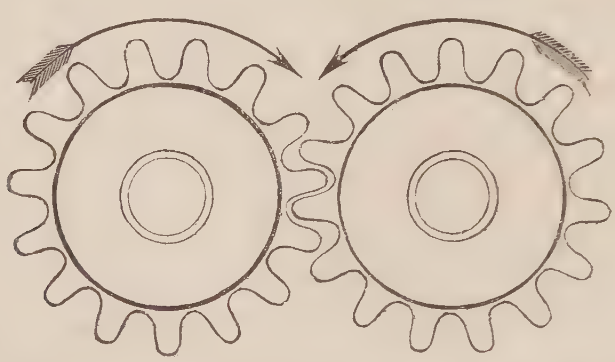

FIG. 2. DIAGRAM showing the cylinders of the comminuting machine in profile ; three-quarters the natural size.

When a portion of the liver substance is passed between the rollers of this machine, it is crushed at once into a state of far finer comminution than could be effected by any cutting process

with knife or scissors. The greater part is reduced to the condition of a loose granular débris, and the whole of it is so bruised and lacerated that the contact of alcohol or boiling water will instantly affect its entire mass. By this means from 1,500 to 2,000 grains of the liver tissue may easily be separated from the body of the living animal, thoroughly comminuted, and immersed in alcohol or boiling water, within the space of ten seconds.

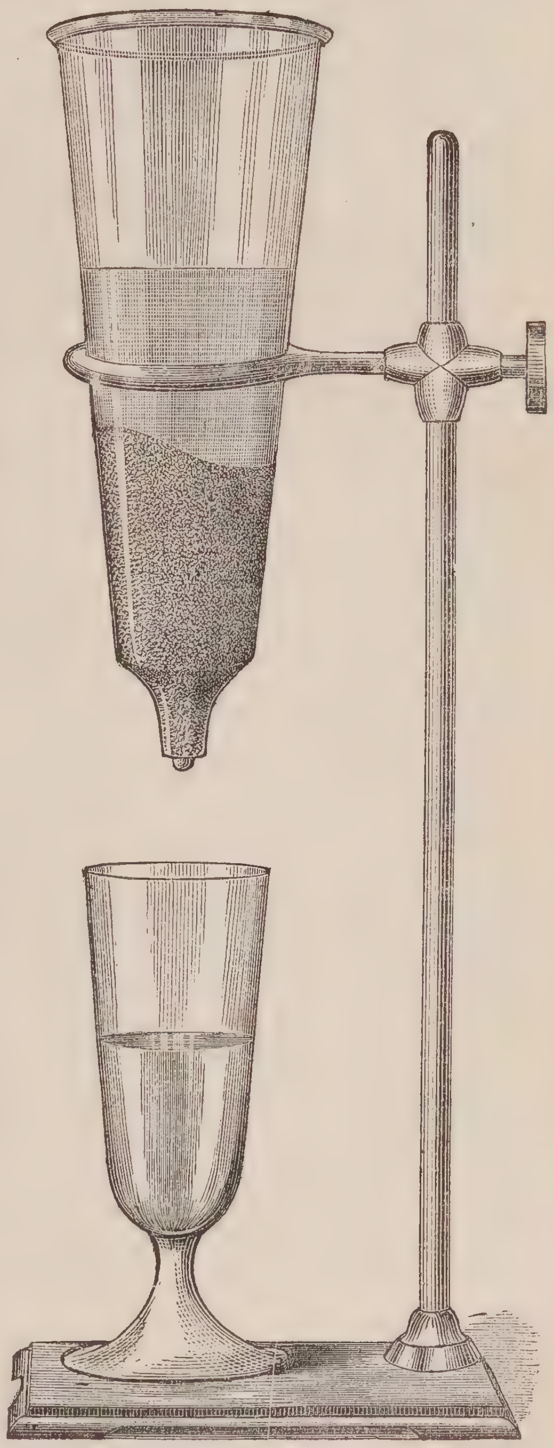

FIG. 3. DISPLACEMENT APPARATUS.

The mode of operating which I adopted is as follows: The animal is gently but firmly held upon a table by three assistants, care being taken to prevent any struggling or any undue disturbance of the circulation or respiration. The animal being secured in this position, and quiescent, the abdomen is widely opened by a single stroke of a very sharp knife, the liver seized and drawn downward, a portion of it instantly cut off and passed between the rollers of the comminuting machine into a vessel containing ten fluid ounces of strong alcohol (specific gravity), .820. A fourth assistant meanwhile marks the time consumed in the operation by means of a stop watch, the second hand of which is liberated at the instant the portion of liver is cut off,

and again stopped when it is comminuted and immersed in the vessel of alcohol.

After remaining for ten minutes in the alcohol, the liver tissue is pounded and ground to a pulp in a porcelain mortar, the pulp thoroughly mixed with the same alcohol, and the mixture then slowly percolated in a displacement apparatus. The moist liver tissue remaining in the apparatus is pressed in a linen bag, and the expressed liquid filtered and added to that which has already passed through by percolation.*

The alcoholic solution is then mixed with an equal volume of water, the turbid mixture clarified in a displacement apparatus with two ounces of coarsely powdered animal charcoal, and evaporated to dryness over the water-bath.† The dry residue is finally dissolved in 50 cubic centimetres of water, again decolorized with one ounce of finely powdered animal charcoal, and filtered. The perfectly clear and colorless watery solution is now examined for sugar, by placing one cubic centimetre of the liquid in a narrow test tube, against a black ground, adding one drop of Fehling's solution, and raising the mixture to the boiling-point.

In order to determine the proportion of sugar present in the liver tissue, one cubic centimetre of Fehling's solution, diluted with water to five times its volume, is placed in a large-sized test tube against a white ground, and raised to the boiling-point. While the ebullition continues, a measured quantity of the liver extract is slowly added, drop by drop, until all the copper ox-

* In some of the first experiments, more alcohol was added in the percolating apparatus, until twenty ounces in all had passed through ; but this was subsequently found to be unnecessary, since ten ounces will extract all the glucose contained in the quantity of liver tissue usually employed, or, at least, sufficient to give a well-marked sugar reaction. The addition of a larger quantity of alcohol is even detrimental in one respect, as it dissolves out more fatty matter, and thus renders the subsequent clarification of the liquid more difficult.

† A more direct method would be to evaporate the alcoholic solution to dryness, and immediately dissolve the residue in water. But if treated in this way the final watery solution is very turbid, and its clarification with animal charcoal is a process of extreme difficulty. The operation is greatly facilitated by proceeding as indicated above. The best way is to mix the alcoholic solution directly with the animal charcoal, and afterward to add the water. With careful management, the liquid will then pass through perfectly clear.

ide present has been precipitated, and the remaining liquid, when filtered, has no longer any blue color. The composition of Fehling's solution is such that, to accomplish this result with one cubic centimetre of the test liquid, it requires exactly .077 of a grain of glucose; which quantity was accordingly present in the portion of liver extract employed. From this is calculated the amount of glucose in the whole fifty cubic centimetres of liver extract, representing a certain quantity of liver tissue; and thence the proportion of sugar, per thousand parts, in the liver tissue itself. The quantity of liver substance, used in each experiment, was ascertained by comparing the volume of the comminuted liver tissue and alcohol with that of another, previously weighed, portion of liver, treated in the same way and mixed with the same quantity of alcohol.

A second method is to employ boiling water, instead of alcohol, for the purpose of preventing post-mortem fermentation. The portion of liver cut out, in the manner already described, is passed through the comminuting machine directly into a vessel containing thirty fluid ounces of water in active ebullition. With this quantity of water, the temperature of the mixture, on immersing the bruised liver, does not fall at any time below 210° Fahrenheit, and actual ebullition recommences immediately. The boiling is continued for five minutes, after which the liver tissue is ground in a mortar to a fine pulp, and again mixed with the same water. The mixture is next boiled down to about two fluid ounces, mixed with five times its volume of alcohol and filtered; after which the alcoholic solution is treated as in the former process.

In each case it is essential that the final watery solution be absolutely clear and colorless. Otherwise, in testing for small quantities of glucose, it is sometimes difficult to be certain whether a genuine reduction have taken place or not; and especially in using the volumetric method for quantitative determination, the extraneous matters present interfere with the test, and prevent our fixing the precise point at which all the copper of the test liquid has been reduced.

I have now experimented in this manner upon twenty dogs. In four of the cases, the method employed was that by boiling water; in the remaining sixteen cases, that by alcohol. The

animals were examined four, eight, twelve, and twenty-four hours after feeding; the food consisting always of the fresh or cooked meat of the bullock's heart. The longest time which elapsed, from the separation of the liver to its immersion in the alcohol or boiling water, was 13 seconds; the shortest time was 3 seconds. The average time was $6\frac{1}{4}$ seconds. In every instance the final watery solution gave a decided and perfectly unmistakable sugar reaction, amply sufficient, in all cases in which it was attempted, to allow the quantitative determination of the glucose by the volumetric method. The proportion of glucose in 1,000 parts of the liver tissue was thus ascertained in one-half the cases. In the remainder its presence only was determined, without regard to actual quantity.

The following is a list of the experiments, with their results:

Experiment.	Time after Feeding.	Time consumed in taking out Liver.	Process of Treatment.	Proportion of Glucose in 1,000 parts of Liver.
No. 1.	4 hours.	13 seconds.	Alcohol.	Glucose.
No. 2.	4 hours.	7 seconds.	"	"
No. 3.	4 hours.	10 seconds.	"	"
No. 4.	4 hours.	4 seconds.	"	"
No. 5.	8 hours.	7 seconds.	"	"
No. 6.	8 hours.	6 seconds.	"	"
No. 7.	4 hours.	5 seconds.	Boiling water.	"
No. 8.	12 hours.	6 seconds.	" "	"
No. 9.	12 hours.	8 seconds.	" "	"
No. 10.	12 hours.	5 seconds.	" "	"
No. 11.	4 hours.	9 seconds.	Alcohol.	2.093
No. 12.	4 hours.	5 seconds.	"	0.804
No. 13.	8 hours.	7 seconds.	"	1.750
No. 14.	8 hours.	3 seconds.	"	1.510
No. 15.	12 hours.	5 seconds.	"	1.810
No. 16.	12 hours.	5 seconds.	"	4.175
No. 17.	12 hours.	7 seconds.	"	1.830
No. 18.	12 hours.	3 seconds.	"	4.375
No. 19.	24 hours.	5 seconds.	"	3.850
No. 20.	24 hours.	4 seconds.	"	2.675

In no single instance, in these experiments, was Fehling's test employed for the detection of sugar, without a similar quantity of the test liquid being boiled at the same time, to make sure that it was not liable to spontaneous precipitation. The volu-

metric method was applied with every care, the liver extract being added very slowly, and the fluid, after precipitation, being nearly always filtered twice, in order to determine the exact point at which its decolorization was complete.

There is no doubt that the quantity of glucose in the liver increases immediately after death ; although, according to my experiments, this increase is not always so rapid as might be inferred from the general statements of some writers. In the following cases, its quantity, per thousand parts, was determined at various periods after death :—

Proportion of Glucose in the Liver.

	At the end of	*Per* 1,000 *parts.*
Exp. No. 15....	5 seconds	1.810
	15 minutes	6.792
	1 hour	10.260
Exp. No. 19....	5 seconds	3.850
	6 hours	11.458
Exp. No. 20....	4 seconds	2.675
	1 hour	11.888
	4 hours	13.361
	12 hours	15.351

It might perhaps be doubted whether the glucose thus existing in the liver at the moment of death may not be due to the arterial blood with which the organ is supplied, rather than an ingredient belonging to the hepatic tissue. This, however, is not the case. The question may be settled by examining, at the same time with the liver or immediately afterwards, some other abdominal organ equally well supplied with arterial blood. The spleen was selected for this purpose ; and in three cases was taken out within ten minutes after the excision of the liver, and treated in precisely the same manner by the alcohol process, with the following result :—

Proportion of Glucose per 1,000 *parts.*

	At the end of	*In the*	
Exp. No. 14....	3 seconds........	Liver.....	1.510
	10 minutes........	Spleen....	0
Exp. No 19....	5 seconds........	Liver.....	3.850
	10 minutes........	Spleen....	0
Exp. No. 20....	4 seconds........	Liver.....	2.675
	10 minutes........	Spleen....	0

In the last-mentioned experiments, the watery extract of the spleen, treated in the same way with that of the liver, gave no reduction whatever on boiling with Fehling's solution; but in each case, after being concentrated over the water-bath to one-tenth of its volume, it yielded an uncertain sugar reaction, entirely too small for quantitative determination.

From these results I think we may legitimately draw the following conclusions:—

I. Sugar exists in the liver at the earliest period at which it is possible to examine the organ after its separation from the body of the living animal.

II. The average quantity of sugar existing in the liver at this time is at least two and a half parts per thousand.

III. The liver sugar thus found does not belong to the arterial blood with which the organ is supplied, but is a normal ingredient of the hepatic tissue.

I am greatly indebted, for valuable aid in the course of the foregoing investigations, to Professor Chandler, of Columbia College, and to my assistants, Dr. George B. Fowler, Dr. John G. Curtis, Mr. Frederick W. Chapin, and Mr. George Hart.

Dr. Detmold inquired if Dr. Dalton could account for the absence of sugar by other experimenters? The tendency of the

paper is that others have not made the experiments in the same manner.

Dr. Dalton, in reply, said that in his first experiments he, and probably others, had arrived at the same conclusion, that there was no sugar present. He supposed that at first it was owing to the small quantity of tissue used. He then attributed his success to the large quantity of tissue used, but now finds it when only a small quantity is used. Very thorough comminution of the tissue and throwing it immediately into alcohol and then percolating it are essential steps in the operation. The strong alcohol coagulates the other substances, but allows the sugar to remain in solution. A liberal use of animal charcoal in filtering the solution is necessary.

A watery solution is apt to be turbid and not easily filtered through the charcoal, especially when the quantity is small, as a quantity of the sugar is likely to adhere to the charcoal.

Any one can detect sugar in a very short time if this process is carefully followed.

The President inquired if there was any apparent reason for the variation in quantity.

Dr. Dalton said he attributed the variation to individual peculiarities, as some individuals eat more than others.

The liver cannot be examined without finding sugar. After all the sugar is washed out, it will reform, and that process will go on until all glycogenic matter is exhausted or till putrefaction commences.

The President said the formation of sugar in the liver is a result of disassimilation, and ought to be going on all the time.

Dr. Gouverneur M. Smith, being called upon by the President, remarked as follows on the uses and derangements of the glycogenic functions of the liver :—

I have listened with great interest, Mr. President, to the elaborate paper on "Sugar Formation in the Liver," which has been read to the Academy this evening by Dr. Dalton. Being engaged more especially in the study of the practical branches of medicine, I scarcely feel like acceding to the invitation to take part in the discussion of a question so purely physiological as the one which has just been presented.

As, however, it has so happened that my attention has been somewhat directed to the subject of glycogenesis and production of the various proximate principles in the vegetable kingdom, and have laid some inferences upon these points before this body in a paper on vegetable assimilation, about two years and a half ago, and as I have also recently called your attention to the generation of sugar in the animal economy during lactation and in persons suffering with diabetes, I will perhaps venture to add a few words on this occasion, in the hope of turning the debate in a practical direction, but without diverging from the subject legitimately under consideration.

It seems to be proved, by the experiments of Bernard, Flint, Lusk, and Dalton, that the liver possesses the property of generating saccharine material. This animal sugar can be traced from its source to the right side of the heart and to the lungs, but in these emunctories it for the most part disappears; in the blood of the general circulation it can scarcely be detected.

A question which is at the same time physiological and relevant to the one under discussion, is the use of this sugar which is found at all abundantly, at least, in such a comparatively small portion of the vascular system? This point has not been alluded to this evening, nor do I know whether or not physiologists have come to any definite conclusions concerning it. Until we are furnished with more light on this subject, we are incompetent to elucidate various morbid conditions which appear to me possibly to have a more or less dependence upon varying supplies of hepatic sugar.

Is the sugar a mere product of disassimilation, as some have argued, in this respect like urea, but unlike it in the fact that sugar is not an excretory material, and can be usefully appropriated in the economy? Sugar is certainly very different in character from the ordinary products of disassimilation, such as we generally regard them, as for example creatine, creatinine, etc. Sugar is taken as food *per se*, as are also other principles readily convertible into it. Beneficial results doubtless follow such ingestion, for there seems an instinctive craving for the materials of the kind just mentioned.

In vegetables we observe various metamorphoses of organic matter, it being the final resultants which are utilized in assi-

milation. In the animal kingdom the sugar may in part be directly appropriated. Another part may be regarded as a principle which is an intermediate one, in some similar chemical series of transformations as occurs in plants, the ultimate products playing important and conclusive *rôles* in the economy. Sugar, being a familiar substance and comparatively easily detected, has been isolated and made an object of special interest as formed by the liver. It seems to be most probable that sugar is a product rather of the chemical metamorphoses of materials preparatory to assimilation, than a result of organic detrition.

Now, in whatever light we regard hepatic sugar, whether as a result of disassimilation—which is a view, as has been stated, that some have advocated—or as a distinctive product so to speak, we have yet to trace its special influence upon the animal economy.

Nor is this amount of animal sugar so insignificant as to be unworthy of notice. It is constantly being generated irrespective of the chemical nature of the aliment, though its formation seems more liberal if the pabulum has been amylaceous and saccharine in character. According to the careful experiments of Dr. W. T. Lusk, the blood of the right side of the heart, in carnivorous animals, contains from a quarter to half a grain of glucose per fluid ounce under normal circumstances. Though such a large proportion of sugar does not occur in the blood of the general circulation, nevertheless, as the supply of hepatic sugar is so constant, we can infer that the amount formed during the course of twenty-four hours amounts to no inconsiderable quantity.

Fibrin, according to Lehmann, is present in the blood in the proportion of only 4.05 in 1,000, but this modicum suffices to subserve most important uses in the animal economy. A small percentage may consequently, under certain conditions, yield an unexpectedly large aggregate. The amount of urea detected on analyzing the blood seems almost infinitesimal; but when this product of disassimilation is gathered by emunctories and excreted, its measure is by no means trivial, varying, according to Parkes, from 286 to 688 grains *per diem.*

As, therefore, sugar is so constantly formed in the liver and

in appreciable amounts, it must have a purpose in the economy of sufficient moment to deserve our special investigation; but the thoughts now hurriedly offered on the subject must merely serve as texts for a more extended and future consideration.

Derangements of the glycogenic function of the liver have been almost entirely overlooked as causes of disease, with the exception of the attention directed to the subject in reference to diabetes. In the various forms of functional and organic disorders of the liver, morbid conditions have been attributed to abnormal qualities of the biliary secretion, while they may have been at least in part due to a defective supply of saccharine material. If the liver during health is constantly generating sugar, such production is for some beneficent purpose. This function is as liable to interruption as are the other vital operations, and interruptions of physiological processes induce morbid *sequelæ*.

A *diminished* formation of sugar in the animal economy must be characterized by as remarkable abnormal phenomena as those which mark a too bountiful generation of saccharine material. In diabetes we preclude, at times, the use of sugar and of proximate principles liable to conversion into that body, for the reason that the system seems already surcharged with it. There is doubtless an opposite condition to this in which the animal organism suffers from an insufficient supply of sugar, and such condition, so far as I am aware, has not attracted any very special notice. Patients suffering under a natural deprivation, of the kind indicated, can to a certain extent be relieved by artificial means in the same manner as we can supply deficiencies of iron, of phosphorus, etc.

It is reasonable to suppose that hepatic glycogenesis may, under peculiar circumstances, be either arrested or so modified that the supply of sugar in the economy may be greater or less than in health. The symptoms indicating such conditions must vary in degree according to the intensity either of the interruption or augmentation of the function.

Now, in order that we can more thoroughly appreciate the pathological states just alluded to, we must better comprehend the physiological uses of the glycogenic function of the liver.

It may not be inappropriate, in this connection, to review for

a moment the uses of sugar in the vegetable kingdom. It is not proposed at this time to speak of the method of generating sugar and other proximate principles in full growing plants, for this topic I have presented on a former occasion and illustrated with chemical diagrams.* The subject can be most readily studied in examining the growth of embryotic plants.

The seeds of plants ordinarily contain a large amount of starch, which material is chiefly stored either in the cotyledons or in the surrounding tissues. In some seeds the fixed oils are found abundantly as a substitute for starch, and subserve a similar purpose; but on this point it is unnecessary to enlarge. When these seeds containing starch are placed under circumstances favoring germination, the starch is converted into dextrine and thence into sugar. The latter being soluble is easily absorbed by the cotyledons, radicles, and plumules of the embryo.

The sugar thus appropriated is disposed of by the plantlet in two ways. One part is converted into acetic acid, which in turn is transformed by oxidation into carbonic acid and water. An *evolution of heat* attends these processes of vital chemistry, and the young plant is thus early enabled to maintain a suitable degree of temperature. Another part of the sugar is converted into permanent tissue, either into cellulose, $C_{24}H_{20}O_{20}$, which is isomeric with sugar, or into some other structure composed of the ultimate inorganic elements just enumerated.

This brief exposition must suffice to show the chief uses of sugar in the vegetable economy, and if the botanists have been correct in their interpretation of the phenomena characterizing assimilation and the production of heat in plants, we are perhaps partially furnished with a guide to the study of the uses of sugar in the animal kingdom. It is scarcely necessary to add that while it is possible to follow certain parallels of similarity between the organic kingdoms of Nature, we should bear in mind that in some essential functions they are as diverse as they are congeneric in others.

A few words now in regard to the physiological uses of the glycogenic function of the liver, in the hope of throwing light

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* Bulletin of the N. Y. Acad. of Med. Vol. III.

upon some pathological and other conditions which may be attributable to variations in the function.

In the paper on Diabetes, which I read before this Academy in February last,* a divergence was made from the subject for a moment in order to suggest a new office for the liver during lactation. A brief repetition of the general view then expressed may not be inappropriate to this occasion as illustrating one phase of the topic under consideration.

After citing some instances of the increased production of hepatic sugar induced by certain reflex influences, the following theory was advanced. It is well known that sugar occurs in milk in the largest proportion of any of the organic lacteal proximate principles. It was suggested that this sugar was not formed *de novo* by the breasts, as has been contended by physiologists, but was elaborated by the liver and simply separated from the blood and appropriated by the *mammæ* as lactose, possibly after having undergone a slight transition. The glucæmia under such circumstances, however, must be greater than ordinarily occurs. Such increased glucæmia was thus accounted for. The *mammary irritation* accompanying the latter part of utero-gestation and occurring during lactation (and even uterine irritation occasionally) could initiate a peculiar impression which was conveyed to the brain and thence reflected to the liver, inducing an augmented generation of sugar. In other words, there was a nervous connection which was at least sympathetic. In proof that uterine irritation could occasionally induce such condition, I have cited two cases, both patients dying from diabetes. Mammary irritation during lactation doubtless normally excites augmented hepatic glycogenesis, and this increased influx of sugar into the circulation, which under morbid circumstances would be chiefly eliminated from the blood by the kidneys, as in diabetes, is in this instance mainly appropriated by the breasts and made subservient to the nourishment of the nursing child.

Simon, several years ago, noted that milk was richer in sugar early during lactation than at later periods. This phenomenon can now, perhaps, thus be explained. The breasts when about

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* Transactions of the N. Y. Acad. of Medicine. Vol. III.

to assume activity, and during the first few months of lactation, are in a condition better calculated to excite the peculiar erethism referred to than at a later time when the secretion is fully established, and when the breasts partake more of the character of those glands the functions of which are permanent.

Without here repeating other details, in order to test the truth of my assumption, an analysis was made for me of the blood and of the urine of a healthy nursing woman; the former was found to contain sugar, the latter none of this ingredient. While the theory appeared to me to be both plausible and probably true, this single analysis, though corroborative (others would have been made but for the difficulty of procuring specimens), was not sufficient to assure me that the theory was founded in fact, though other circumstances which were given seemed additionally conclusive. A few days after reading my paper I met with a statement, which I here subsequently offered, which appeared to substantiate the view before expressed. M. Blot had frequently noted a concurrence of a transient glycosuria and lactation, but so far as I am aware had not accounted for any such coincidence further than there appeared to be a connection between the phenomena. If M. Blot has been accurate in his observations, not only is an additional fact added to strengthen my theory, but also an easy explanation of the relationship between the melituria and lactation which he has noted. Nature provides such a liberal amount of hepatic sugar during lactation that more is generated than is required by the breasts for its lactose, and a part is consequently eliminated by the kidneys, as in morbid conditions.

While it is unsafe to accept as a scientific fact any theory which is not sustained by incontestable proofs, nevertheless with the information at present in our possession we are justified in offering the above as one of the probable uses of the glycogenic functions of the liver.

It remains to consider the use of the function as it is being ordinarily and constantly executed.

Sugar, as has been stated, can be readily traced from the liver to the right side of the heart and to the lungs; in the blood of the general circulation its amount, as physiologists assure us, is quiet infinitesimal. Is this sugar chiefly destroyed in the lungs

and eliminated as carbonic acid and water, an *evolution* of *heat* attending the metamorphose? Such disposition of it in the animal economy would in its chemical aspects correspond with that recognized as occurring in the vegetable kingdom, though the process is not identical. In fully developed plants the carbonic acid, whether derived from the air, soil, transformation of sugar, or otherwise, is for the most part retained and decomposed to procure carbon and liberate oxygen; but the plantlet growing from the seed destroys the sugar and evolves a small amount of carbonic acid.

If sugar is disposed of in the lungs, after the manner indicated, we have a partial explanation of the method of maintaining animal heat. The temperature of the centric parts of the body is somewhat higher than that of the peripheric portions, and such fact is perhaps to be thus accounted for. This difference, however, is not very material, and the extremities for various reasons are more liable to be cooled than the trunk.

It has occurred to me that there is another and important way in which the hepatic sugar may be made use of in the economy. Is not a part only disposed of in the manner indicated, and while a small portion is retained in the circulation to be used, as will be hereafter shown, is not another and considerable portion converted while in the lungs into some other principle, some *tertium quid* which the physiologists have not recognized? This unknown principle may pass on into the blood of the general circulation, and in its various coursings be either broken up and evolved through the skin as carbonic acid and water with the evolution of heat, and thus aid in maintaining the heat throughout the body, while another portion may be converted into tissues composed of its elements, carbon, hydrogen, and oxygen.

Dextrine, in plantlets, is the intermediate material between starch, out of which it is formed, and the sugar which is recognized as being finally destroyed and disposed of as before described. Sugar in the animal economy may be regarded in a somewhat analogous view; a part of it being intermediate between the material out of which it is formed in the liver and some unknown substance into which it is converted in the

lungs, to be finally disposed of in the blood of the general circulation.

It appears to me, Mr. President, that the conclusions herewith given respecting the several uses of the glycogenic function of the liver are legitimately drawn from the scientific premises which at present are in our possession. I have purposely omitted here all allusion to the formation of sugar in the economy otherwise than in the liver.

A final question of interest in this connection remains to be studied, namely, what morbid phenomena characterize derangements of the function we have been considering? To the production of sugar in the economy as occurring in diabetes, I shall not allude; for this topic has recently occupied the attention of the Academy at two of its sessions. The phenomena accompanying melituria are so remarkable that they can readily be recognized. There are doubtless various other abnormal conditions depending upon deviations from the normal formation of sugar.

In various functional and organic diseases of the liver, if the biliary secretion is impaired, it is reasonable to suppose that there may be also a concomitant derangement of the glycogenic function. The biliary and glycogenic functions may both be disturbed in the same individual, or again it may happen that one function may be disordered while the other is not interfered with.

Assuming that the uses of sugar in the animal economy are such as have been given, it may be legitimately inferred that if hepatic glycogenesis is diminished or arrested, patients suffering under such condition should have a temperature below the normal standard, and should emaciate from the want of a ternary principle to convert into tissues of similar chemical composition. These symptoms would be most apparent and of primary occurrence unless an artificial and chemically adjusted diet prevented their appearance; other phenomena may occur of secondary incidence, being *sequelæ* of a prolonged deprivation.

Now, in persons suffering from various chronic affections of the liver and even of other viscera, the liver being secondarily involved, we find both the conditions before enumerated. Even

in confirmed diabetes, where there is a surplus of sugar, the same occurs, for the sugar is chiefly excreted through the kidneys, the deranged system not appropriating enough to maintain a uniform and normal temperature and to prevent emaciation.

There are many individuals who are not bed-ridden invalids, but who are hypochondriacal and dyspeptic; in such the continual sensations of chilliness and the loss of flesh, etc., may be partially attributable to a failure in the liver to produce a sufficient quantity of sugar. In various disorders in which there may be a sympathetic or secondary disturbance of the liver, we may trace similar symptoms to the same cause. And, again, an ordinary lean physique may sometimes indicate a diminished glycogenic function, and not simply a defective power of assimilation. It may happen also that a slender person may suffer intensely during warm weather and have an unusual tolerance of cold; in such a one an error of assimilation prevents a due appropriation of saccharine material as fat, and the greater part of the sugar is destroyed with the evolution of heat.

I have been led to suppose that there may be an *increased hepatic glycogenesis without* a necessary occurrence of *diabetes.* Can we not thus, at times, account for *obesity?* Sugar may be morbidly generated and become largely assimilated as fat, and consequently not be excreted by the kidneys. Such view seems to be plausible and probably true. We occasionally observe a transient melituria in corpulent persons,—the sugar is formed more generously than can at times be assimilated. These cases of ephemeral diabetes either correct themselves, so to speak, or can readily be relieved by dietetic means. It sometimes happens, however, that the assimilative powers no longer can appropriate the excessive saccharine matter, and the patient emaciates and falls into confirmed polyuria. I have met with such latter instance in a person of full habit. Dr. Wm. Roberts, of Manchester, states that he has seen diabetes in corpulent persons. Of the causes of these variations in the formation of liver sugar, time forbids me to speak at length at this time.

On a former occasion it was my aim to show the relationships which existed between certain nervous conditions and increased hepatic glycogenesis. There may be opposite conditions in which the nervous system is so prostrated that it is unable, either

temporarily or permanently, to stimulate even the normal amount of glycogenesis. But on this point I cannot at present enlarge.

Thus far allusion has only been made to the effects of differences in the *quantity* of sugar generated by the liver. There may be variations in the *quality* of the glycogenic function which occasion unfavorable *sequelæ*. Either an abnormal form of sugar is elaborated, or some material which is not sugar, and which, though allied to it, is a diseased product, and fails to supply the economy with a material susceptible of being utilized in a healthful manner. The function is not arrested, but simply perverted.

There may be another mode not before alluded to in which either a saccharine or an allied material may indirectly prove a source of disease. It is well known that sugar, when artificially acted upon by ferments, undergoes a peculiar metamorphosis. It is within the range of possibility to suppose that sugar or some congeneric but abnormal product, as formed by the liver and carried into the blood when the system is laboring under various morbid conditions, may be metamorphosed into principles which, if not positively inimical, are at least unsuited to supply the requirements of certain vital processes.

The subject we have been considering this evening has numerous *direct* and indirect bearings. Time forbids me, however, to allude to others at present, though some of them are, perhaps, as important as are any of those to which reference has already been made.

I have ventured, Mr. President, to offer these few thoughts which have occurred to me in considering the subject of sugar-formation in the liver. It has seemed appropriate to bring forward, this evening, several relevant topics, both physiological and pathological in their nature, concerning some of which, perhaps, no very decided opinions have been formed. The thoughts herewith offered have been hurriedly gathered, and must be considered as being merely introductory to a more mature consideration. The uses and derangements of the glycogenic function of the liver have received comparatively little special attention. It remains for us to give to these subjects further study, and in the spirit of philosophical inquiry en-

deavor to remove any misconceptions concerning them which we may have entertained, and to enlarge our views respecting an interesting group of normal and abnormal phenomena.

STATED MEETING, JULY 6TH, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

Dr. W. F. THOMS exhibited an apparatus for ventilating rooms. It consists of a square box to which is connected a tube on the top, another on the bottom, and another on one side; and by means of a valve in the box the fresh air, entering through the side tube, may be conducted through the upper tube to the top of a room or through the lower part of the room, according as it may be needed for summer or winter use. In either the upper or lower tube, an hour-glass shaped apparatus, made of wire, for holding ice for cooling the air, or substances for medicating it, may be placed.

Dr. STEPHEN SMITH then read the paper for the evening, on "Fractures and Dislocations of the Odontoid Process."

Dr. HOWARD inquired of Dr. Smith, at what age fracture of the odontoid process occurs most frequently.

Dr. Smith, in reply, said it occurs most frequently from 20 to 30 years of age, and in the proportion of 19 males to 3 females in the cases reported in his paper. The extremes were 6 and 60 years of age.

Dr. JOEL FOSTER inquired if lifting a child by its head, as is sometimes done, would not be likely to produce a fracture or dislocation of the odontoid process?

Dr. Smith said that one or two of the cases referred to in his paper were supposed to have been caused in that way.

Dr. HOWARD remarked that it is generally supposed that in hanging there is a fracture or dislocation of the odontoid process, but in several cases where he had made post-mortem examination after hanging, he had never detected any fracture or dislocation; death had been caused either by strangulation or apoplexy.

Dr. Peaslee said, after the experiments made by Dr. Smith, we cannot believe that it can occur frequently unless there were disease of the bone before.

He thought a projection of the arch of the bone in the posterior pharyngeal space would aid in the diagnosis.

Dr. Smith said that in the cases referred to in his paper the fracture was not in the anterior arch, but in the posterior arch, near the base of the odontoid process, and that the process itself was seldom fractured unless it were diseased.

Dr. Post had seen one case, a child whom he supposed had a fracture of the odontoid process, but was not able to follow the case, and did not know the result.

The President, through the courtesy of Dr. Ceccarini, presented a paper entitled "Preservation of the Cow-pox in Fossil Pustules," and some specimens of vaccine crusts, sent to the Academy by Vincent Luis Ferrer, M. D., of Havana, Cuba, with a request that they be referred to a committee to test his plan, which is a new one, and he claims better than any other known for preserving the virus in its full efficiency. His plan is to dissect out the vesicle from the heifer on the sixth day, dry it carefully, and cover it with tin-foil. He claims that it can be kept for a long time and in any climate, and, when moistened and pulpified and applied to scarifications involving the dermis, acts with great certainty and energy.

The subject was referred to the Committee on Vaccination, appointed on a former occasion; and a vote of thanks to Dr. Ferrer was passed for his valuable and elaborate communication.

The Academy then adjourned.

Stated Meeting, September 21, 1871. Dr. W. C. Roberts, Vice-President, in the Chair.

Drs. C. M. Bell, Edward Frankel, and Joseph W. Howe, were elected Resident Fellows.

Dr. C. A. Leale read a report from the Section on Diseases of Women and Children.

Dr. S. T. Hubbard related a fatal case of poisoning from hydrate of chloral. The patient, a female, about twenty-eight years of age, was not under his care, and had no physician. He was informed, on his arrival at the house, that she had been advised to take it by a female friend for the purpose of procuring sleep. The label of the bottle was marked, "Hydrate of Chloral ʒ ij, in water ℥ iij. Tablespoonful to be used as directed." The patient had taken about ninety grains.

Dr. Roberts remarked that there was uncertainty in the doses of chloral, and the time when to use it, and he hoped the gentlemen would give their views in regard to it.

Dr. Stephen Rogers said it did not appear to him that Dr. Hubbard could report this case as an authenticated one of chloral-poisoning, for no *post mortem* was made of the brain, head, etc. In regard to symptoms of chloral-poisoning, he recollected, when the wife of a Fellow of the Academy was poisoned with it, the symptoms resembled those of poisoning by belladonna. This lady took sixty grains on the first day, eighty or ninety grains on the second day, and during the first nine hours of the third day one hundred and fifty grains, when symptoms of poisoning manifested themselves. There were delirium and unconsciousness for several hours, with no ability to respond after partial consciousness had returned, though she knew what was going on about her.

Dr. Roberts called attention to its accumulative power.

Dr. J. C. Peters exhibited several maps showing the course taken by cholera during its different epidemics, and predicted that it would become epidemic in New York next spring. It requires about two years for cholera to go through Asia, on account of the want of railroads and canals.

An address of welcome was read by the Vice-President, Dr. W. C. Roberts.

Dr. S. Rogers read certain resolutions on Abortion, which were adopted.

The Academy then adjourned.

STATED MEETING, OCTOBER 5, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DR. W. T. LUSK was elected a Resident Fellow.

Dr. S. WATERMAN, by invitation, then read the following paper.

SPECTRUM ANALYSIS APPLIED TO MEDICAL SCIENCE.

BY S. WATERMAN, M. D.

MR. PRESIDENT AND FELLOWS OF THE ACADEMY:

LITTLE did I dream, when I appeared three years ago before the American medical profession, with a *résumé* of spectrum analysis up to that year, that my studies and investigations would attract the attention of your honored body, representing so ably the medical profession of the metropolis; a body whose very name is expressive of scientific culture, and in whose sacred halls the most erudite speak with subdued enthusiasm and measured diction.

Let me assure you, gentlemen, I realize both the honor conferred upon me by the invitation extended to me by your honored President and also the grave responsibility to enlarge before you upon a subject at once so attractive and of such magnitude. My object is, and has been, to bring spectrum analysis prominently before the American public The greatest physicists in Europe have readily acknowledged its high importance and usefulness. Its accomplishments in chemistry and astronomy may be considered as among the highest triumphs of the human intellect. The material accumulated up to this present hour, and bearing directly upon the domain of the medical profession, is intensely interesting: it accumulates daily and hourly, and it is safe to say no man can claim to stand at the height of medical culture who is ignorant of the results of spectrum analysis. Much of this material is directly applicable to the practice of medicine. Much of it has contributed to enrich physiology, pathology, animal chemistry, and botany; while its bearing upon medical jurisprudence

makes it imperatively necessary that its voice be heard, and its revelations heeded, where blood is to be identified, poison to be demonstrated, and the mode of death by irrespirable gases is to be proved.

But it is not the mere quantity of result that gives spectrum analysis its superior value, nor how much of it may be directly employed in the practice of medicine. Science establishes facts, and leaves it to practical minds to apply them usefully. By spectrum analysis we understand a scientific process, in which light, solar or artificial, is made use of to analyze and demonstrate substances, both organic and inorganic. The instrument used consists of a system of prisms and lenses, by means of which rays of light are broken up into a series of colored tints called a spectrum, and which offers to the eye all the colors of the rainbow with extraordinary beauty and brilliancy. The instrument employed is a spectroscope. You are all familiar with the triangular piece of glass called a prism; you all know that a ray of light passing through it undergoes certain changes. In the first place, the figure of the ray itself is greatly enlarged; it spreads out to the left and to the right as it emerges from the prism—it disperses. We call this refraction. But the ray of light undergoes another remarkable change—it is decomposed, and, instead of white light which enters the prism on one side, we behold colored lights emerge from the opposite side, running down from red to orange, yellow, green, blue, and violet. The spectrum obtained by artificial light we call a continuous spectrum, in contradistinction to the spectrum obtained from sunlight, or the light of the moon and stars, which is called an interrupted spectrum. When we look upon a spectrum thus obtained from sunlight through a narrow slit in the shutter of a dark room, or one provided for in the spectroscope, we observe, when properly focused, that the spectrum is traversed by a number of fine lines, which have been counted and mapped, and, according to Angström's table, there are more than twelve thousand of them. The most distinct of these lines were first discovered by Wollaston in 1802. Frauenhofer,

however, made first use of these lines for purposes of exact measurement, as they recur, in all spectra obtained by aid of sunlight, with the utmost regularity. He designated them as A, B, C, D, E, F, G, and H, and these lines have since been known as the Frauenhofer lines. Here you see these lines, and you will at once understand what we mean by speaking of a continuous or an interrupted spectrum.

You observe this line in D, from which the most marvelous discoveries of modern times had their beginning. It is also called the sodium-line, and, under strong light and the combination of several good prisms, it is resolved into two and even four lines. This line, produced by burning sodium, is coincident with the D line of the sun-spectrum. Prof. Kirchhoff, of Heidelberg, investigated this curious coincidence. The only difficulty presenting itself was that the sun-line was a dark line, while the line from burning sodium was a bright-yellow line. The philosopher, however, found means to reverse these lines, and, by a remarkably ingenious reasoning, demonstrated that both had a common origin, and that therefore there was sodium in the sun in an incandescent state. I can but glance at these things this evening.

The dark lines are all produced by substances in the highest state of combustion in the sun. The E line results from iron; C, F, and G are produced by hydrogen, of which there seems to be an incomprehensibly large quantity in the sun forming solar columns of fire, which, by solar currents, are carried sometimes a hundred thousand miles upward. The line C is the magnesium-line, and, according to Angström, the H line is produced by burning calcium.

Prof. Stokes was the first who instituted physiological researches with the spectroscope. He published his results in 1864, and described and mapped two highly-interesting absorption-bands, characteristic of arterial blood, or blood fully charged with oxygen, and the bands of a derivative from blood-hæmatine. He also mapped and described the broad band of blood deprived of its oyxgen, as well as the reduced hæmatine bands.

The process of making an analysis by means of the spectroscope is simple enough. Bodies to be examined are either solids, fluids, or gases. The solids must be brought to a state of incandescence by the compound oxygen-flame, or by the electric arc, when they send rays of all degrees of refrangibility. Fluids are placed before the slit in suitable glass vessels with plano-parallel walls; a common test-tube will often serve our purpose very well. Gases are examined by means of an electric induction-spark, which passes through their extremely attenuated atmosphere. The apparatus employed consists of a thin-bored thermometer-tube, with two bulbs or balls at each extremity, into which electrodes of platina or aluminum are inserted. The tube is filled with the gas to be examined, and then exhausted with the air-pump until only the $\frac{1}{600}$ or $\frac{1}{700}$ part of the ordinary pressure of the atmosphere is left. In that state of attenuation the gas offers very slight resistance to the electric spark. It becomes very hot, emits brilliant and beautiful light of various colors, changing, of course, with the different gases employed.

Thus, no known substance can defy the analytical power of the spectroscope. Every known metal, alkali, or alkaline earth, when heated up to a gaseous state, gives off light peculiar to itself, influencing and changing the spectrum in a manner peculiar to itself and to none other—some of them absorbing all the colors of the spectrum, with the exception of a single bright band, of which sodium and thallium furnish an example; others are known by many fine bright lines in various parts of the spectrum, as barium, cæsium, and rubidium. The fluids show dark bands in the spectrum of various depths of shading, breadth, and purity of outline. These dark bands are called absorption-bands. Of the fluids capable of being spectroscopically analyzed, none is so rich in distinct features, and none has attracted more deserved attention, than the spectrum of the blood, of which we shall speak presently.

One word regarding the extraordinary delicacy of the spectrum-test, which far surpasses every other test previously known to us. To give you an idea of the sensitiveness of this

test, let us take one pound of common table-salt and divide in 500,000 parts. One of these minute atoms of matter is called a milligramme. The experienced chemist is able to weigh such a minute particle only with the most delicate scales, and with extraordinary care and acquired dexterity. But, with this performance, he has arrived at the limits of possibilities. And now let us divide again one of these particles into 3,000,000 parts, and we obtain an atom of matter so minute that the human mind is incapable of forming any conception of it. Yet we can demonstrate its presence by the spectrum-test with the utmost certainty and ease. The dusting of a book in the remotest corner of the hall will immediately cause the flashing up of our old acquaintance, the sodium-line, and thus reveal the presence of this metal in the dust.

The delicacy of reaction is not confined to sodium alone. Lithium gives a reaction with a $\frac{9}{1000000}$ part of a milligramme, strontium with $\frac{6}{400000}$ parts of a milligramme; from the ash of a cigar, moistened with hydrochloric acid and held in a flame, we obtain, simultaneously, the spectra of sodium, potassium, lithium, cæsium, rubidium, and calcium.

The marvelous analytical power of the spectroscope has been evidenced in the discovery of several new metals. Two, cæsium and rubidium, by Bunsen and Kirchhoff, who recognized 200 grains of the mixed metal in 44 tons of the waters of Turkheim; one by Crooks, to wit, thallium, one by Reich and Rutter, of Freiburg, Germany, called indium.

The facility to observe the sodium-line in the spectrum is of great importance to the physicist. The presence of this salt in sufficient quantity in the air is absolutely necessary to life in its many and different forms and conditions. Its antiseptic properties are specially adapted to prevent contamination of the air, earth, and water, and its deficient supply is probably connected with the rise and progress of endemic contagious diseases, such as cholera, typhus, and others. The never-failing source of this salt is the sea; fine particles of it are continually floating in the air, resulting from the action of winds and storms, and from the slower processes of evaporation.

Thus we meet the compensating power of natural laws. The storm that brings destruction to the shore-bound mariner casts abroad, over the continents, the elements necessary to life and health; and, while life may be sacrificed to its relentless fury, its saving mission to millions endowed with life testifies to the greatness and wisdom of the Creator.

Daily spectroscopic observations show considerable variations in the supply of this salt in the air, by corresponding variations in the intensity of the brilliancy of the sodium or D line. We may thus be able to foretell hereafter an approaching epidemic and its departure, with the same exactitude as we are now able to foretell the approach and cessation of a storm.

Of the various metals spectroscopically demonstrated in the human body, we may mention potassium, sodium, lithium, rubidium, cæsium, and calcium. Lithium, also, emanates from the sea; it permeates many soils, and is found in many plants, and in the milk of animals feeding upon these plants.

Rubidium and cæsium are constituents of many mineral springs. The celebrated waters of Bourbon le Bains, the springs of Vichy, Gastein, Neuheim, Carlebrunn, and others, owe their curative powers to the presence of these salts. Important in a practical point is the spectroscopic test of barium and thallium. In cases of poisoning with these salts, it is the easiest and most reliable test to determine its presence. The objection, that the many modifications of the spectrum, by so great a number of the substances as can be spectroscopically examined, are liable to lead to confusion, is devoid of foundation. It is, on the contrary, a fact that those who become acquainted with the various spectra by observation do not need the exact measurement of the single bands or lines, in order to detect the various constituents. The color, relative position, peculiar form, variety of shape, and brightness of the bands, are quite characteristic to answer ready recognition and exact results, even in the hands of persons unaccustomed to work with the spectroscope.

As the exhaustive analysis of blood by the spectroscopic

test forms the basis of its applicability to the diagnosis and the treatment of disease, I will rapidly glance over the array of facts known in reference to this interesting subject.

The first paper on the relations of light to the coloring pigment of the blood was published by Stokes, in 1814, and afterward verified and enlarged upon by Hoppe Seyler. Stokes pointed out the facts that the blood causes a peculiarly strong absorption of light in the yellow and green part of the spectrum, that blood exists in a double state of oxidation, *that each state was characterized by its own peculiar modification of the spectrum*, that the primitive coloring pigment of the blood could be altered by chemical agents, and that its altered condition forming hæmatine, it would induce corresponding optic changes quite different from those of oxidized blood.

In the mean time, experiments were made with hæmato-crystalline, the crystallizable substance of the blood. It was soon found that it sustained the same relations to oxygen as the blood does, that it was capable of abstracting the oxygen from the air and parting with it under the influence of the same physical and chemical agents; and that it showed also the same optic relation, and that any alteration this substance undergoes causes a corresponding change in its spectral properties also.

As this hæmato-crystalline, or hæmoglobin as it is sometimes called, plays so very important a part in spectrum analysis, I will mention a few of its many remarkable properties. In form it belongs to the rhombohedral system. It is the most indestructible body known. It retains its integrity for years, in solutions that have undergone the offensive process of putrefaction. It retains, also, its optic relation to light; and its peculiar bands, it is reported, can be reprodued from mummies that have departed life thousands of years ago. It is of high atomic weight, computed by C. Schmidt and Hoppe Seyler to be as high as 13,280.

The stability and indestructibility of this substance are principal factors in the stability of an individual's life; and as the quantity of hæmato-crystalline ebbs and rises in the animal

economy, so rises and falls the capacity for life and health. The absorption of oxygen is effected with the utmost rapidity; and Pflüger's experiments on dogs showed also that this vital gas is consumed with equal rapidity in the economy.

One of the most valuable properties of the spectroscope is its ability to determine the quantity of hæmato-crystalline, as well as the quality; and thereby to ascertain the amount of vitality of an individual in a given case. We can do this in various ways, which being matter of detail I am forced to pass by. One of these ways is to use smoked glasses of known obscuratory powers—a procedure of my own, which I shall publish at some future day.

The possibility to measure the vital force is certainly a matter of the greatest importance. True, we can measure the temperature; we can count the pulse, and by practice learn to appreciate its oscillations, its volume, and other peculiarities. But is the pulse or the thermometer absolutely reliable? What experienced physician has not witnessed in inflammatory conditions, such, for example, as peritonitis, meningitis, etc., a compressed, small, wiry pulse, which would lead the unwary to administer stimulants, to the imminent danger of the patient; when after a moderate abstraction of blood the pulse would quickly rise to great strength.

Let us confess we are often in doubt, whether a case is truly the result of anæmia or otherwise; forcing us to temporize when the fleeting moments are extremely precious.

How welcome to us is, in such moments, the information which the spectrum-test is so well able to impart, resting upon strict scientific principles, and based upon Nature's immutable laws! What in former days was revealed to the most favored observers can now be ascertained by a simple knowedge of spectrum analysis.

Spectrum analysis is suggestive as to the proper treatment of abnormal conditions, depending upon permanent or temporary alteration of the blood-crystals. We understand now how it happens that, when a man has inhaled the poisonous and much-dreaded fire-damp, the mephitic exhalations of the

coal-mines, for example, he may be brought to the surface alive, may linger on for days, and yet is beyond the possibility of recovery, even if he were plunged into an ocean of oxygen. Such was the condition of many of the victims of the late accident in the collieries of West Pittston, Pennsylvania. In these cases the crystallizable ingredient of the blood had been affected. We know now that the act of breathing is not a mechanical but a chemical act, that the hæmato-crystalline alone possesses the marvelous capacity to attract and fix the oxygen, loosely, indeed, so that it may as easily be exchanged for carbonic acid; that the blood in the lungs saturates itself with the vital oxygen, is carried by the heart's action into the remotest recesses of the capillary system; that it there parts with its precious burden and supplies the oxidizable tissues with the breath of life; and that it carries back the irrespirable carbonic acid, to be exhaled by the action of the lungs, assisted by chemical processes not yet fully understood.

We know the strange and fatal affinity of hæmato-crystalline for carbonic oxide and other irrespirable gases, which, once attached to it, form an inseparable alliance, hold it in deathly embrace, use up all oxygen, so necessary to the animal economy, to satisfy their own wants, as is the case when sulphuretted hydrogen is inhaled; or the deathly messenger deprives the hæmato-crystalline of its power and capacity to absorb oxygen, and to convert the hæmato-crystalline, as is the case when carbonic oxide has been inhaled, or both effects transpire at once, when, for example, prussic acid has been brought into the circulation.

In these cases transfusion is the only rational remedy; the infusion into the vitiated current of a fresh supply of hæmato-crystalline to sustain anew the vital exchanges. The quantity need not be large. Such are the philosophical deductions which spectrum analysis forces us to accept. Formerly, when poisoning took place with irrespirable gases, efforts were made to neutralize their effects by chemical antidotes. It was taken for granted that the same chemical affinities would be exercised in the human economy as in the laboratory; and

medical men recommended the inhalation of sulphuretted hydrogen as an antidote in cases of poisoning with chloric gas—and so the reverse. Unconscious of the utter hopelessness of such proceeding, they thus heaped danger upon danger.

Thanks to our progressive agè, such destructive practice lies far behind us.

I have spoken of transfusion, in these and other cases *where the vitality of the hæmato-crystalline has been suspended or destroyed.*

This operation is not free from danger; it requires proper mechanical means not accessible to all, and may not be on hand when wanted. It also requires experience, which not every physician may be able to gather.

In view of this, I propose to give the hæmato-crystalline internally, not alone in poisoning with gases, but also in cholera and typhus, which affect the integrity of this life-sustaining substance, to substitute it for blood, or to inject it, hypodermically.

It can now be purchased in quantity, and experiments should be instituted to test the correctness of my proposition, which is at least logically and philosophically correct. As this substance alone possesses the respiratory power, it may prove superior to transfusion. Its indestructibility would insure its reaching the circulation in an unaltered condition. But even should it undergo a chemylatic change, under the influence of the gastric juice of the stomach, its only possible transmutation would be into hæmatine, which substance, in common with hæmato-crystalline, also possesses the breathing power of the blood, although, perhaps, in an inferior degree. I shall be in possession of a quantity of this salt from abroad, and will test the value of my suggestion by actual experiments.

Now, all these changes of hæmato-crystalline, as effected by irrespirable gases or chemical means, are traceable upon the spectrum by corresponding changes of bands and lights. True as the needle to the north-pole is the spectrum to the substance it analyzes.

These two well-defined bands in the yellow and green part

of the spectrum, between D and E, are the world-renowned blood-bands of blood (*see* diagram) in a state of oxidation. The arterial blood is in this condition. If we deprive the blood of its oxygen by reducing agents, these two cheerful bands disappear and make room for this one broad band between D and E. The two bands will, however, reappear if the blood is shaken up with air or mixed with substances capable of giving up oxygen.

If blood or hæmato-crystalline is acted upon by any acid, or alkali, or alcohol, it is chemolized into albuminous subtances and a colored matter which retains all the iron, but none of the sulphur, of the original compound. This is hæmatine. It we place this substance before the spectroscope, we obtain a quite different spectrum, or, better, a number of spectra, the modification depending upon the agent that has been employed to separate it from the original compound.

Here on the diagram is a four-banded spectrum of hæmatine, obtained by the action of acids upon the blood.

The one below is a five-banded hæmatine spectrum, obtained by the action of acidulated alcohol upon blood.

Here is a one-banded hæmatine spectrum. It is obtained by the action of a pure alkaline upon blood. Hæmatine may also be reduced, like hæmato-crystalline, by reducing agents. If thus reduced it presents the following spectral bands, resembling the blood-bands, but differing in position, width, and depth of shading.

Hæmatine is of rare occurrence in the economy. It has been detected ready formed in the urine, in a disease to which the name "intermittent hæmaturia" has been given, and I have once found it in the last stage of Bright's disease.

A new body, termed cruentine, has been discovered, and described by Thudicum. It is derived from hæmatine by the action of sulphuric acid upon a portion of the material employed, and which remains insoluble. This substance, like hæmatine and hæmato-crystalline, can exist in a double state of oxidation. It can be oxidized and deoxidized at pleasure, and the simple shaking up of the mixture with air is suffi-

çient to bring it back to a state of oxidation. This derivate or decomposition product of the second order also possesses the respiratory power of the blood-corpuscles.

As in the case with hæmatine, cruentine has its own spectral peculiarities, and offers a rich harvest to the spectroscopist. We have some modifications of cruentine, a few of which I have presented upon the diagram. You observe that they differ from each other and from the spectra of hæmatine as well as hæmato-crystalline, in features quite distinct and readily recognizable. These modifications are dependent, as was said of hæmatine, upon the chemical agent employed, and are matters of detail which I am admonished to discard this evening.

Being in possession of all modifications to which blood can be brought by chemical agencies, we are now prepared to understand how this analysis can be applied in medicine.

First and foremost its adaptation to forensic medicine may be considered. No matter in what manner blood-stains have been tampered with, be it by maceration, boiling, acids, alkalies, or alcohol, the spectroscope can tell us all about it. Where no change has been attempted, we can show the well-known two blood-bands. Where boiling has been resorted to, we know that the hæmato-crystalline has become coagulated, and we must obtain the hæmatine tests. So when acids and alkalies have been employed, we may use the cruentine reaction with its characteristic bands. We have already adverted to the fact that hæmato-crystalline preserves its integrity almost forever, and that we can always demonstrate it spectroscopically. You may interrupt me by claiming that a good microscope will demonstrate blood equally well. To which, however, I must respectfully oppose my demurrer. I am no stranger to the microscope, and know, in the first place, that the defining power of the best instrument falls far below the response of the spectrum-test. But it is entirely useless when blood has been acted upon by the various chemical agents above enumerated, and where the corpuscles have been disintegrated and destroyed, leaving no characteristic by which

the microscope can definitively demonstrate blood. At this juncture the marvelous power of the spectroscope is shown to its greatest advantage, and the best authorities abroad have frankly admitted that no blood-analysis is complete without the application of the spectrum-test.

In cases of poisoning by prussic acid, carbonic oxide, sulphuretted and phosphoretted hydrogen, the spectroscope is able to give decided reactions, provided the blood can be examined soon after death; and although the absorption-bands of blood affected by prussic acid and carbonic oxide do not reveal any very striking deviation, in relation to spectral appearances, from those of pure solutions of hæmato-crystalline, yet we have ample means to recognize them by the spectroscope. For example, carbonic-oxide hæmato-crystalline resists the action of reducing agents, which blood does not, and Dr. Gamgee considers this sufficiently characteristic to determine in forensic cases poisoning by carbonic oxide.

Spectrum analysis has thrown light upon the nature of bile and disease, and made us acquainted with many of its results of decomposition. Normal bile is nearly devoid of the power to affect the spectrum, and this negative quality becomes a moment of great diagnostic value.

The presence of the biliary acids was formerly demonstrated by means of the "Pettenkofer test." A mixture of sulphuric acid and sugar with these acids produces a splendid purple color. The test, however, is no longer considered a valid one, for cerebric acid, lithofellic acid, and even albumen, give this test with rare intensity. The spectroscope alone is able to give reliable information, and we possess optic differences by which each of these bodies may be recognized.

Satisfactory results have been obtained from spectral analysis of the urine. The presence of hæmato-crystalline or hæmatine in the urine is, of course, easily detected, for, in diseases where an extensive destruction of blood-corpuscles takes place, I have repeatedly discovered a well-defined band near F at the commencement of the blue part of the spectrum. When this band becomes visible in the urine it indicates gravity of

disease, and is a landmark to the practitioner, informing him that the vital powers of his patient are breaking up and fast ebbing away.

A year ago I predicted prismatic peculiarities, could purpurine be spectroscopically examined. Want of material prevented me from discovering and describing two very distinct and beautiful absorption-bands between D and C and E and F; they were discovered by Stokes. As this pigment found in the urinary secretion, derived from a peculiar morbid transmutation of the biliary acids, indicates grave hepatic disease, such as cirrhosis, contracted and indurated liver, hypertrophy of the spleen, and ascites depending upon hepatic disorder, the early discovery of this pigment is of the greatest diagnostic value. By the spectrum-test we can now discover it in the smallest quantity. The absorption-bands presented on the diagram result from an alkaline solution of purpureum.

The spectral appearance of urine in endemic *cholera deserves special notice.* The early urine of patients under the influence of this terrible plague contains a peculiar principle, which has the property of generating a blue coloring-matter or pigment, when *acted upon by nitric acid and heat.* This urine pigment gives a peculiar absorption-band, quite different from the blue pigment in urine which is derived from iudicace. It differs also from the blue pigment derived from bile, but bears resemblance to the alkaline alcoholic solution of hæmatine. We call this pigment urocyanical. Under the action of certain chemical agents, a red pigment is also obtained from urine of cholera patients, characterized by its own peculiar absorption-band. We call this pigment, urorubric; it resembles the band of hæmine. The spectrum-test may, therefore, be applied to diagnosticate true endemic cholera from the urine of cholera patients. In addition to the spectral phenomena of cholera urine, we have peculiar absorption-bands characteristic of cholera stools, the so-called "rice-water dejections." The spectrum resembles that of blood, but differs from it sufficiently to distinguish one from the other for practical purposes. These cholera bands are not produced by any other

evacuations known, and we have, therefore, in them a diagnostic sign of the highest value, to distinguish in doubtful cases sporadic cholera from the true Asiatic disease. The source of the pink pigment in cholera stools is probably the hæmato-crystalline split up by the destructive chemolitic power of the cholera-poison.

The spectroscope has also thrown considerable light upon the cause of some symptoms in Bright's disease, hitherto little understood. We all know that uncontrollable vomiting and retching is often a constant symptom in chronic renal disease. This is probably produced by two new bodies discovered by Thudicum. They are decomposition products, derived from urochrome, to wit, omicholine and omicholic acid. They are formed within the economy in certain kidney-diseases. They have highly-nauseating and emetic properties, and probably cause that uncontrollable irritation of the stomach, especially in cases where, by an irrational treatment, salutary evacuations have been suppressed, and the burden to relieve the system of this noxious product has fallen upon the stomach.

These bodies can be readily demonstrated by the spectrum-test. Uromelanine, a decomposition product from urine, is also sometimes found ready secreted in the urine and can be easily detected by the spectrum-test. It is often coincident with melanotic disease, and points at grave disordered assimilation and imperfect decarbonization of the blood. A substance always found in connection with it is paramelanine. It is an interesting body, *as it yields the spectroscopic characteristic of cruentine, thus revealing its origin.*

Spectrum analysis has recently been employed to demonstrate minute quantities of vegetable poisons and their alkaloids, such as strychnine, veratrum, atropine, morphine, and others. Metallic poisons, such as arsenic, copper, antimony, lead, thallium, and barium, can be most readily detected by bringing them into a state of incandescence, when each of them has its own peculiar modification of the spectrum. Many of the solutions of narcotic poisons and their tinctures give characteristic absorption-bands, and by the aid of the spectro-

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scope we can detect adulterations of wine, malt and fermented liquors; by its aid we also determine the age of wines, and the quality of fixed and volatile oils.

Recently the spectroscope has been employed in ophthalmic practice to determine states of color-blindness and other abnormal conditions of the retina, and to determine the capacity of colored glasses for spectacles, suitable to individual cases, where only a certain amount of light is borne and desirable.

A new spectral test for albumen is recorded in the *Microscopical Journal* of the Royal Microscopical Society of July, 1870. Certain algæ, when decomposing, give up a blue fluid, called by Cohen, phycocyan. Under the spectroscope it has one absorption-band, unless it has been decomposed in the presence of albumen, when it has two bands. When to this colored fluid, giving one band, is added a minute quantity of albumen, it produces a second band. This is considered by Dr. Hogg one of the most delicate tests for albumen known.

Other interesting material having a purely physiological bearing, the identity of luteine from birds and mammalia, the various spectra of intestinal discharges of sucking infants, the blue and green pigments resulting from morbid intestinal oxidation and other interesting topics, must be left to some future evening's consideration. I have already transcended the limit of time allotted to me. I have necessarily omitted much that is interesting, and avoided all detail. May my efforts stimulate others, placed in more favorable positions, to take up the subject, and give it that critical consideration which its importance so richly deserves.

Dr. Charles P. Russell asked Dr. Waterman if he could, by spectral analysis of the stools, detect sporadic cholera from true Asiatic cholera.

Dr. Waterman thought he could. The pink absorption-band was always found in the urine of true Asiatic cholera patients. If this pink absorption-band can't be found on spectral analysis of the urine, it can be put down as sporadic cholera.

Dr. J. C. Peters inquired what was the substance which produced this absorption-band.

Dr. Waterman said it was hæmato-crystalline, broken up by choleraic disease.

The President remarked that the coloring-matter of the blood is obtained from the coloring-matter of the disease. He also said that it would not be more wonderful, at some future time, to detect typhoid fever, than to diagnose typhoid fever now. Its application to astronomy was mentioned.

Dr. Peaslee stated that one grand conclusion is arrived at, viz., that every thing in Nature is pervaded by a proportion of chloride of sodium.

Dr. J. C. Peters remarked that it is quite reasonable to presume that a peculiar epidemic constitution of the atmosphere may be induced by an excess or deficiency of sodium, ammonium, iron, and ozone. But, when we are told that a considerable lessening of the amount of sodium in the air may cause wide-spreading epidemics of cholera and typhus fever, we must pause and see whether there are not many well-known facts which disprove this supposition.

We need, for instance, only to call to mind the great number of instances in which cholera has been conveyed across the sodium-salt ocean; has broken out in mid-ocean, and has raged in numberless seaports, the whole air of which is loaded and impregnated with muriate of soda.

Dr. Burrall said that those who have written on cholera have stated that chlorine did not act on cholera-poison.

The President, in closing the discussion, remarked that the question, as it had been presented by the author of the paper, was narrowed down to this: that a total absence of sodium might produce cholera.

Dr. A. H. Gallatin, by request of the President, then read a paper on the same subject; which was as follows:

NOTES ON THE APPLICATION OF THE SPECTRUM ANALYSIS TO THE STUDY OF THE CHEMICAL CONSTITUTION OF THE BLOOD.

Being a Review of our Knowledge on that Subject, consisting chiefly of literal Extracts and Translations from the Works of Claude Bernard, René Benoit, Paul Bert, Ernest Hardy, and W. Bird Herapath.

BY ALBERT H. GALLATIN, A. M., M. D.,

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HISTORICAL NOTES.

HOPPE in 1862 was the first to study the blood with the spectroscope. He saw the characteristic bands of its absorption spectrum, and examined the modifications which this spectrum undergoes under the influence of different reagents.*

Valentin in 1863 studied the phenomena of luminous absorption, not only for blood but also for bile, in a great number of animal and vegetable coloring-matters, and especially in poisonous substances. He thought these phenomena would be of service for the solution of certain problems in legal medicine.†

Sorby, in 1864, began his endeavors to create from observations of absorption spectra a true method of qualitative analysis, particularly applicable to coloring-matters, and entirely independent of ordinary chemical analysis. He studied and classified systematically the action of different reagents in spectra; he established general laws, and invented a new apparatus, the "*micro-spectroscope.*" ‡

* Hoppe: Virchow's "Archiv für Pathologische Anatomie," 1862, xxxiii., 446.

† G. Valentin: "Der Gerbrauch des Spektroscops zu physiologischen und ärtzlichen Zwecken," 1863.

‡ Sorby: "On a Definite Method of Qualitative Analysis of Animal and Vegetable Coloring Matters by Means of the Spectrum Microscope." (Proceedings of the Royal Society of London, 1867, xv., 433.)

Stokes, in 1864, founding his opinion on the optical phenomena presented by biliverdin and chlorophyll, which had already established a remarkable relation between the phenomena of absorption and the phenomena of fluorescence, denied the identity which was thought to exist between these two substances.* The same observer a short time after concluded that the change of color that the blood undergoes in the capillaries at one place, and in the lungs at another, is due to an alternating reduction and oxidation of its coloring matter.†

In 1867, Askenasy added to our knowledge of the nature of chlorophyll, and of similar coloring-matters. ‡

Gamgee, in 1868, wrote on the action of the nitrites on the blood.§ The author shows that the color as well as the absorption spectrum of blood undergoes change when acted on by nitrites. The two sharply-defined absorption-bands of the oxidized coloring-matter become very faint, and an additional though faint band appears in the red. If the blood thus altered be acted upon by ammonia, the color changes from chocolate-brown to blood-red again, and the absorption-band in the red disappears, and the two bands between D and E again become visible.

Bird Herapath, in 1868, first uses the micro-spectroscope in medico-legal inquiry. The case was Reg. *vs.* Robert Coe, Swansea Spring Assizes, 1866. In this case nearly all traces of blood stains had disappeared from the weapon employed by the murderer, in consequence of the hatchet having been

* Stokes: "Uber die Unterscheindung Organischer Körper durchihrer optischen Eigenschaften." (Pogg. Ann., cxxvi., 630.) Stokes: On the Supposed Identity of Biliverdin with Chlorophyll, with Remarks on the Constitution of the Chlorophyll." (Proceedings of the Royal Society of London, 1864, xiii., 144.)

† Stokes: "On the Reduction and Oxidation of the Coloring Matter of the Blood." (Proceedings of the Royal Society of London, 1864, xiii., 355.)

‡ Askenasy: "Beiträge zur Kentniss des Chlorophylls, und einiger dasselbe begleitenden Farbstoffen." (*Botanische Zeitung*, 1867, Nos. 29, 30.)

§ Philosophical Transactions 1868, p. 589.

left exposed in the woods for several weeks after the deed was accomplished. It was only on the removal of the head of the hatchet that any appearances of blood were to be obtained from the surface of the handle, which had been protected by the iron ring, and on carefully making thin sections of these stained portions of wood, and treating them with distilled water, a few drops only of a brownish-colored fluid were obtained, which coagulated and became discolored on boiling; also another drop when placed in a very minute tube about half an inch long and one-eighth of an inch in diameter, the total contents of which tube were one grain and one third of distilled water, gave the optical absorptive bands due to old blood. (It is brown hæmatine which is usually discovered in old blood-stains; but red hæmatine in dry and more recent blood-clots.) This little drop of bloody-colored fluid was placed on the stage of the microscope, and examined with an inch Ross objective, illuminated by an achromatic condenser, and the micro-spectroscope was inserted into one of the tubes of a binocular microscope, as an ocular lens would be employed. This form of instrument is that known as the Torby-Browning spectroscope, and it admits of great precision, as it has a lateral spectroscope, as well as a terminal one. These two spectra appear side by side in the field of view, and, being perfectly parallel, admit of examining substances by two sources of light at the same time, or enable us to make comparisons between two different or similar substances at the same time and by the same kind of illumination—the two spectra being both visible with the same eye. This form of instrument is very sensitive to small quantities of blood. In the case just described, conclusive results were produced with a quantity of blood not exceeding in weight the one-thousandth part of a grain. The justness of the sentence was afteward proved by the confession of the prisoner previous to his execution.*

* "On the Use of the Spectroscope and Micro-spectroscope in the Discovery of Blood-Stains and Dissolved Blood, and in Pathological Inquiries." (*The Chemical News;* English edition, xvii. No. 431, pp. 113, 114.)

Notes on the Absorption Spectra of Blood.

1. It gives a characteristic spectrum in the normal state.

2. The coloring-matter which it contains, under the influence of appropriate reagents, undergoes certain modifications, which can be translated by corresponding transformations in its spectrum. Hence, blood possesses five or six characteristic absorption spectra, any one of which is sufficient to recognize its presence.

3. In preparing the blood-film, a drop may be pressed flat between two glass plates; but it is much better to dilute a drop with distilled water, and place it in a glass tank whose width can be regulated with a screw. Water rapidly attacks the exterior envelope of the globules, and becomes red by dissolving without altering their coloring-matter. Let the blood coagulate, then crush the clot, wash with water and filter. We then obtain a colored aqueous extract, very diaphanous, and which has all the optical properties of blood.*

4. In studying arterial blood, it may be drawn directly from the artery, or any blood may be saturated artificially with oxygen.

5. We may conclude that the blood, as it is when issuing from a living being, presents a specific absorption spectrum, characterized by two dark bands in the yellowish-green part, and by the nearly complete extinction of the more refrangible rays, to the disappearance of the blue or of the indigo. Also, that there are slight differences between the optical properties of arterial and venous blood.

6. All blood, no matter what its origin, always gives the same specific spectrum. (Of course, an exception must be made in the case of white-blooded animals.) Hoppe first affirmed this †. Valentin proved it by studying the blood from man, the horseshoe bat, the dog, the new-born and adult cat, the rabbit, rat, and heron; from the terminal vein of the embryo

* Stokes: "On the Reduction and Oxidation of the Coloring Matter of the Blood." (Proceedings of the Royal Society of London, June, 1864, xiii., p. 355.)

† Virchow's "Archiv," 1862, xxiii., p. 446.

of a chicken three days old; from the adder, perch, tadpole, and frog.* M. Paul Bert arrives at the same conclusion. M. René Benoit could not find an exception to this law. He studied the blood from man, the ox, the sheep, the dog, rabbit, and frog.†

7. If we treat with water very old blood-stains, left on iron, wood, or linen, etc., we can get the characteristic spectrum. Nevertheless, under certain circumstances, the coloring-matter can undergo, while drying, alterations which modify its spectrum. Benoit states that he found the characteristic bands in blood which he had left freely exposed to the air for several months, which gave an infectious odor and which the microscope showed to be invaded by the infusoria of putrefaction.‡ Another authority mentions a case where he detected the blood-spectrum in a solution prepared by acting with distilled water on a chip from a dissecting-table, that had not been in use for three years. In a case that came before Prof. John C. Draper, in the winter of 1870–'71, it became necessary to examine a club with which a murder had been committed. Here both microscope and spectroscope failed. Clearly all traces had been dissolved out and washed away by the snow, in which it had lain all winter.

8. With regard to the sensibility of the method. Valentin has shown that the characteristic spectrum can be recognized in an aqueous solution containing $\frac{1}{7000}$ of blood.

Notes on the Nature and Optical Properties of Hæmoglobine.

1. Hæmoglobine is the coloring-matter of the blood. At first it was only known by its products of decomposition, hæmatine or hæmatosine. It was first obtained pure and crystallized by Leydig and Kölliker, in 1829. In 1851, it was regarded by Funk as a substance found only in the blood of the rat, and the same year it was discovered by Kunde, in the

* Valentin: "Der Gerbrauch des Spektroscops zu physiologischen und ärtzlichen Zwecken," 1863, p. 87.

† "Études Spectroscopiques sur le Sang; par René Benoit," 1869, p. 60.

‡ Ibid., p. 62.

blood of a great number of animals. It was then prepared by Lehmann from the blood of the dog and Guinea-pig. It was not well understood until after the labors of Hoppe-Seyler, Schmidt, Valentin, Stokes, Rollett, Lang, Preyer, etc.

2. It is known in two states, crystallized and amorphous. The crystals belong to the hexagonal and rhombic systems depending on the animal, that of man being rhombic. The solubility of these crystals varies with the species of animal.

Schmidt's Analysis of Hæmoglobine Crystals: $C_{55.64}H_{7.11}N_{16.19}O_{21.02}S_{0.66}Fe_{0.43}PO^{5}{}_{0.91}$.

According to Hoppe-Seyler and Dybkowsky,* hæmoglobine always contains a certain proportion of oxygen feebly held. It presents with different animals differences in constitution.

Preyer proposes this formula: $C_{1200}H_{960}N_{154}Fe_2S_6O_{354}$.

It possesses a slightly acid reaction.

3. Oxyhæmoglobine is the coloring-matter of arterial blood. Hæmoglobine absorbs and loses oxygen with great facility. It seems to be proved that there is a very unstable connection between the two. It would seem as if it were not a true chemical combination, but that the oxygen is held in a state of tension. The blood of all vertebrates contains hæmoglobine, but it is found in only some species of the invertebrates. In the latter it is held in solution in the plasma, as, for instance, in the case of earth-worms and of certain annelides. In other invertebrata the blood is colorless or contains a coloring principle different from hæmoglobine. Lankester has found hæmoglobine especially in the blood of animals which live in fetid mud, in which the water, almost deprived of oxygen, incloses an excess of carbonic acid and even of sulphuretted hydrogen; and he admits that these creatures owe to their hæmoglobine the power of storing up the oxygen necessary to their existence.

4. With regard to reduced hæmoglobine or the coloring

* "Einige Bestimmungen über die Quantität des mit Hämoglobinlose gebundenen Sauerstoffs." (Medicinisch-Chemische Untersuchungen, S. 120. 1866.) Dybkowsky.

matter of venous blood, the change of color produced in blood by circulation, by putrefaction, and by the action of reducing reagents, is explained by the transformation of oxyhæmoglobine into reduced hæmoglobine, and *vice versa.* But whether this transformation be due to a simple absorption of oxygen or an absorption of water at the same time, is undecided. The latter is probably the case, according to the researches of Hoppe-Seyler.

5. Hoppe-Seyler has proved the presence of absorption-bands in the blood during circulation in the living animal. He examined the ear of a man and a white rabbit, the edges of the fingers held adjacent, the palm of the hand and the natatory membrane of frogs. The identity of the spectral phenomena of the coloring-matter of the blood in circulation and of hæmoglobine shows that the latter is the material which gives to blood its color, and not a product of decomposition.

6. The reduced coloring-matter presents particular characteristics when submitted to spectral analysis. A band of absorption between D and E characterizes it. It disappears when the solution is agitated with oxygen or air, and is then found replaced by the two bands of oxyhæmoglobine.

The hæmoglobine of the living blood-globules is never completely reduced. It loses its oxygen by contact with tissues which act as reducing agents, but never in a sufficiently large proportion to give the spectrum of reduced hæmoglobine. In examining with the spectroscope the blood which circulates in a vein, we see the same spectrum as in the case of arterial blood, but veiled. It seems to result from the superposition of the spectra of oxygenated hæmoglobine and reduced hæmoglobine.*

Note on Hæmatine.

The formula of hæmatine is: $C_{34}H_{34}N_4FeO_5$. It is a product of the decomposition of hæmoglobine. It is found in old apoplectic foci, in the intestinal canal, where it is the result of the action of the digestive sugar on the blood, intro-

* "Principes de Chimie Biologique, par le Dr. Ern. Hardy," 1871.

duced as an aliment or excreted by transudation of the vessels, in the fæces, in the urine in certain pathological conditions, etc. Hæmatine gives in the spectral apparatus a band of absorption a little undecided between C and D.*

Crystals of it are found in old hæmorrhagic foci.

Robin gives it the formula: $C_{14}H_9NO_3$.

Mulder's formula for hæmatine is: $C_{44}H_8NO_2$.

Hæmatoidine might be regarded as hæmatine in which one equivalent of iron is replaced by one equivalent of water.

Note on Hæmine.

This substance was obtained by Teichmann in 1853, by transforming the coloring-matter of the blood.

According to Hoppe-Seyler its formula is: $C_{34}H_{34}N_4FeO_5$ HCI. (The chlorhydrate of hæmatine.) It gives by spectral analysis the same bands as hæmatine.

Note on Hæmatine without Iron.

Hoppe-Seyler, by treating hæmatine with sulphuric acid and water, obtained a body which he considered hæmatine without iron. Dissolved in a soda-solution, it gives an absorption spectrum, interrupted by four obscure bands.

Note on Methemoglobine.

Hoppe-Seyler has described, under the name of methemoglobine, a substance which he considers intermediate between hæmoglobine and hæmatine. Its spectrum is characterized by three bands of absorption, of which the last two occupy the same position as those of hæmoglobine; the first to the left is between C and D, nearer C, in a different position from the band of hæmatine in an acid solution. Nevertheless, Kühn thinks that methemoglobine is nothing but oxyhæmoglobine mixed with hæmatine, dissolved by the formic and butyric acids, which form in slight quantity during the decomposition of the first of these substances.

* "Principes de Chimie Biologique, par le Dr. Ern. Hardy," 1871.

Notes on the Transformation of the Boood-Spectra by Reagents.

1. When a blood-solution is treated by different reagents, the characteristic bands change their aspect. These changes are accompanied by alterations of coloration. The bands present new widths and degrees of intensity; they often disappear and sometimes reappear in a different part of the spectrum. Evidently with this must occur changes in the coloring-matter. These secondary spectra are always produced, the same identical ones, every time that the same concourse of circumstances are caused to arise. Hence each one is as specific as that of blood itself, and characterizes a determinate product of decomposition or derivation of the coloring-matter.

2. Stokes proposes "*cruorine*" as the name for the coloring-matter of the blood as it is known to the spectroscopists, viz., such as it exists in the globule in the living animal. Of course the chemist can never examine, otherwise than with the spectroscope, cruorine, as it changes promptly on leaving the blood-vessels or at death. Stokes has investigated the effects of different chemical reagents upon the coloring-matter of the blood,* and has concluded that arterial and venous blood, in the recent fluid condition, contains a substance (cruorine) which, like indigo, is capable of existing in two different states of oxidation and color. In arterial blood, scarlet cruorine is the form in which it is found, and in this condition very dilute solutions produce two very sharply-defined black bands of absorption, and close to and parallel with D, which is the more intense of the two; while the second is found in the green part, about the breadth of the previous band distant from it. On submitting the scarlet cruorine to deoxidizing agents to a moderate extent, the cruorine becomes the deoxidized or purple cruorine, and then the solution is light or deep purple, according to its degree of concentration. Examined by the spectroscope in this condition, only one deep broad band of absorption is found, which commences about D, and then passes onward to the green, absorbing the whole of the yellow

* "Proceedings of the Royal Society," 1864, pp. 355, *et seq.*

and part of the green. If a dilute solution of scarlet cruorine be shaken up with an atmosphere of carbonic acid, the fluid does not exhibit the appearance or spectrum of venous blood. It is evident, therefore, that the blue color of venous blood is not produced by the presence of carbonic acid in solution; but by a *reducing action* in the capillaries analogous to that of other reagents; while the influence of sulphide of ammonium, sulphuretted hydrogen, or the hydrated protoxide of iron, or the protochloride of tin, or the peculiar deoxidation of arterial blood due to the changes going on in the systemic circulation, are all instances in which such a deoxidation or reduction has been in action. A dilute solution of scarlet cruorine, set aside in a full, closely-corked vial, or with very little air, will shortly pass by spontaneous deoxidation to the purple cruorine, and will then exhibit all the optical phenomena of this peculiar substance; but resumes its scarlet color by agitation with air. Prof. Stokes says that of all reducing agents an ammoniacal solution of protochloride of tin (previously treated with sufficient tartaric acid to prevent the precipitation of oxide of tin) is the most efficient reducing agent, and, as it is colorless, it does not interfere with the spectroscopic appearances. He says that when a few drops of this solution are added to a solution of scarlet cruorine, the latter is presently reduced, and we have the spectrum of purple cruorine. If the solution be now shaken up with air, the cruorine is reoxidized to the scarlet form. On standing a few minutes it again becomes reduced, and the solution may be made to go through these changes repeatedly until all the tin has become completely oxidized. But, when blood or scarlet cruorine is treated with an *excess* of deoxidizing material, or has become changed by long exposure to air, or by drying, or by the effect of sulphurous and some other acids, the coloring-matter becomes brown and more insoluble in water. It is then changed into *brown hæmatine*, which has very different optical properties from that of either scarlet or purple cruorine. The two dark bands of absorption are still found; but have become very faint and much less sharp in outline; while a third dark band is seen in the red,

on the less refrangible side of D. This change, when produced by age and exposure, is sometimes months in being fully accomplished, and is only well marked on the appearance of the dark band in the red.* As cruorine can exist in two different forms, so can hæmatine, viz., the brown and the red, the latter being produced by deoxidizing the brown hæmatine by some reducing agent, as hydrated protoxide of iron. These two bands of absorption occur, as in scarlet cruorine; but capable of being readily distinguished from those by their position and different degrees of intensity. In red hæmatine an interval exists between D and the margin of the first band of absorption, which in red hæmatine is less sharp than that in the scarlet cruorine, or than its fellow in the same spectrum; both of which, therefore, are in red hæmatine found in the green. Most chemical reagents convert scarlet cruorine into brown hæmatine without any previous passage through the state and properties of purple cruorine. The effects of reduction on brown hæmatine are evanescent, and the solution rapidly becomes deficient in optical power; it does not assume the former properties of brown hæmatine. The absorption-bands fade away and disappear, the one nearer to D being the more persistent, and remaining sharp during several days. This takes place even if the bottle be air-tight.† Retreatment with protoxide of iron will again reproduce the two absorption-bands as before.

3. It has been proved by the action of certain reagents that there exists in the blood, or in the products of decomposition derived from it, certain substances capable of oxidizing themselves at the expense of the cruorine and of reducing it. What is it that in the living economy tranforms the arterial into venous blood? If it be a reduction, we ought to find in the venous blood the characteristics of reduced cruorine. With regard to the phenomena of coloration, there is a close analogy between venous blood and solutions of deoxi-

* Bird Herapath. *Chemical News*, English edition, xvii., No. 431, pp. 114, 115; No. 432, p. 124.

† Ibid., *loc. cit.*

dized cruorine; venous blood gives not only the reddish-brown tint which belongs to reduced cruorine, but even when in a very delicate film changes color and becomes a greenish shade. This dichroism of venous blood was first observed by Brücke.* This is one of the best ways to distinguish venous from arterial blood.

4. The spectrum of venous blood differs from that of arterial:

(1.) By an absorption of the extreme red.

(2.) By a darkening of the interval which separates the two bands.

(3.) By a greater transparency for the blue rays.†

5. Various reagents produce characteristic blood-spectra.

(*a.*) Carbonic acid, hydrogen, and nitrogen, can expel oxygen from the combination which it forms with cruorine. Benoit says that, in examining sanguineous solutions, through which he had passed a current of carbonic acid, he had seen spectra whose characteristics much resemble those of venous blood. It is necessary to use as little water as possible in making the blood-solution, as carbonic acid is more soluble in serum than in water; water dissolves 1 volume, and serum 1½ volume.‡ These facts prove that carbonic acid can act on the coloring-matter of the blood exactly like a reducing agent. The phenomena which characterize the passage of the blood from the venous to the arterial condition, and inversely, consist in an alternating oxidation and reduction of the coloring-matter. Hence the very important *rôle* which this body, a carriage charged with the transportation of the oxygen from the lungs to the microscopic element of the tissues, plays in the preservation of life.

(*b.*) The fixed caustic alkalies act slowly on the coloring-

* "Wiener Sitzungsberichte," x., 1070, und xiii., 485; also "Pogg. Ann.," 1855, xciv., 426.

† "Études Spectroscopiques sur le Sang; par René Benoit," 1869, p. 77.

‡ Ibid., p. 79.

matter, and the product of the transformation is hæmatosine, which is easily recognized by its optical characteristics. To obtain the spectra of hæmatosine in a distinct manner, it is necessary to take sufficiently concentrated solutions; the process is not as sensible as that for cruorine. Thus, a liquid which gives very sharply defined the bands of this latter, becomes almost entirely transparent when a little ammoniacal alcohol is introduced. (Ammoniacal alcohol, added to a blood-solution, converts the cruorine into hæmatosine.)

(*c.*) Acids modify the optical characteristics of blood. Most organic acids may be mixed with blood without producing a precipitate. If a sanguineous solution be treated by acetic, lactic, oxalic, tartaric, or citric acid, the color passes to a reddish brown, and a profound modification is produced in the absorption spectrum. The two primitive bands disappear completely, and a new and not very intense one is seen nearly bisected by C. The absorption at the most refrangible end retreats considerably toward the violet. The spectrum hæmine is obtained when an acid is caused to act on a sanguineous solution already treated with ammoniacal alcohol; hence, hæmine can be derived, under the influence of acids, from hæmatosine as well as from cruorine. The nature of the acid employed seems to have a very slight influence on the position occupied by the band in the red part of the spectrum. Thus, tartaric acid slightly deviates it towards the left, while citric acid causes it to approach slightly the yellow. But these differences are so slight, that they do not destroy the characteristic value of the spectrum. The band is more intense when hot acid is used. Acids which precipitate the blood produce the same transformation, if they be employed in very small quantity.*

(*d.*) If blood be treated with a mixture of ether or alcohol with an acid, the spectrum is more complicated. Thus, using acetic acid, we see not only the band in the red, but also two other bands in the green, which correspond neither to those of

* "Études Spectroscopiques sur le Sang; par René Benoit," 1869, pp. 81–86.

red cruorine nor those of reduced hæmatosine. If the solution is sufficiently concentrated, the first band is well defined and all the more refrangible part beyond the green is shadowed; but if the liquid be diluted or examined in thinner layers, then slowly the two bands alluded to will become defined in this shadow as it clears up, and at the same time the band in red diminishes in intensity and tends to disappear. Hence, we can never see clearly the three bands simultaneously. Benoit says that, in examining the spectrum produced by the addition of an acid in the blood, he thought he could sometimes see traces of these supplementary bands. It might be thought, then, that they also belong to the spectrum of hæmine, and that is the dissolvant which varies their visibility.* When the blood has been treated by an acid, the neutralization of this acid, by ammonia or an alkaline carbonate, destroys the spectrum of hæmine, but never causes the primitive absorption-bands to reappear.

(*e.*) Hoppe has shown that blood treated by sulphuretted hydrogen presents a characteristic spectrum.† If a sufficient quantity of this gas be passed through a sanguineous solution, it will turn a livid green, which becomes black when the film is very deep. With the spectroscope it gives, besides the two normal bands, which still appear, though a little feeble and undefined, a third band in the red, between C and D, but nearer C, very well defined and characteristic of sulphuretted hydrogen. The absorption is increased for the blue and green, so that the third band is followed by a shadow, so that it is sometimes troublesome to distinguish it. The sulphuretted hydrogen blood-spectrum is of importance in certain medico-legal investigations, since this gas is evolved from the stains of decomposing blood.

(*f.*) It would seem that the blood-spectrum is unaltered by carbonic oxide, protoxide of nitrogen, ether, chloroform, sulphide of carbon, and solution of alkaline and neutral salts.

* René Benoit; *op. cit.*, p. 87.

† Hoppe: "Virchow's Archiv," 1862, xxiii., 448.

APPENDIX.

Notes on Medico-Legal Applications.

1. It is impossible to distinguish the spectrum of human blood from that of animals.

2. By treating distilled water, holding the suspected matter in solution, with appropriate reagents, the spectra of reduced cruorine, of hæmatosine, etc., may be obtained at will; that is to say, we may identify blood by six or seven different characteristic spectra.

3. There is no coloring-matter known which will give a spectrum which an expert wonld confound with that of blood. This point has been denied by Dr. Forbes Winslow, by Steinmetz, and more especially by Paul Bert.* Two substances only have been found comparable in their optical effects to those of hæmatine—a dilute ammoniacal solution of carmine, and a similar solution of cochineal in ammonia, which coloring-matters are very unstable and fade quickly. In both these liquids the same coloring-matter exists, as carmine is produced from cochineal. The two absorptive bands are much broader and more diffuse than any of the optical appearances due to the coloring-matter of blood, though most like those of brown hæmatine, and only a novice could possibly mistake the one for the other, while the least attempt at chemical investigation would pronounce them different; the action of heat alone being sufficient to coagulate the coloring-matter of blood, while the cochineal and carmine would remain unchanged. Reducing agents would also settle the question. Acids immediately change the color of cochineal solution to a reddish-orange deficient in absorptive power, and even a spontaneous coloration will take place in the ammoniacal solution of cochineal. Solutions of carmine are more permanent. Acetic acid produces no change at first, but eventually the coloring-matter

* "Leçons sur la Physiologie Comparée de la Respiration," etc.; par Paul Bert, 1870, pp. 74, 75.

is precipitated. Sulphide of ammonium does not alter it in the least, nor does the alkaline solution of protochloride of tin. On adding protochloride of iron and ammonia to any solution of carmine, the coloring-matter is immediately precipitated in combination with the oxide of iron as a brown or maroon colored compound. But one great safeguard in medico-legal inquiries will be the absence of cochineal or carmine from those positions in which blood may by any possibility be found, some cloth fabrics alone being dyed by a mordanted cochineal. Some scarlet clothes, also, are of this character, and the carmine color, being fixed with alumina, would be insoluble in cold water; whereas the cruorine or hæmatin would dissolve with more or less facility according to its age. It is therefore evident that all these considerations render the detection of blood-stain by spectrum analysis a matter of but little doubt or difficulty, even when in minute quantities.* In the carmine spectrum the bands are much less sharply limited and more refrangible than in the blood-spectrum; the second one is much the darkest and disappears the last; finally, there is no absorption for the violet rays.† Sorby has pointed out two spectra, somewhat like those of blood, viz., both cochineal and orcanette when treated by alum; ‡ both these substances can be distinguished by the different refrangibility of the two bands and by the non-absorption of the violet. Of course, in cases of doubt, we can employ equally well here reagents, and cause the apparition of hæmine or hæmatosine spectra.

4. I have shown that the comparative spectra of cruorine and of a solution of carmine in ammonia have their respec-

* "On the Use of the Spectroscope and Micro-spectroscope in the Discovery of Blood-Stains and Dissolved Blood, and in Pathological Inquiries." By W. Bird Herapath, M. D., F. R. S. *Chemical News*, English edition, vol. xvii., No. 432, pp. 124, 125; No. 431, p. 115.

† René Benoit, *op. cit.*, p. 97.

‡ H. C. Sorby: "On a Definite Method of Qualitative Analysis of Animal and Vegetable Coloring Matters by means of the Spectrum Microscope." (Proceedings of the Royal Society of London, April, 1867, xv., 438.)

tive absorptive bands *colored* and shaded so differently that we can readily distinguish the one from the other.*

5. Blood-stains undergo with time a decomposition which must necessarily act on the coloring-matter and weaken its optical characteristics. Thus they appear less and less glaring as the stains become older. These transformations are always accomplished very slowly; thus Valentin clearly proved the presence of blood on an old wooden dissection-table which had not been used for three years, and which had remained for that time in a moist locality, and on an old rusty hook which had formerly served to hang meat on in a butcher's shop.†

Notes on Physiological and Pathological Applications.

1. Additional light might be thrown on the function of the spleen by examining the spectrum of the splenic vein; we might also discover the origin of the coloring-matter of the bile.

2. By means of its spectrum, blood may be easily recognized in urine, and detected in some forms of albuminuria, even if it be also charged with the coloring-matter of bile. Highly-jaundiced urine absorbs all the blue end of the spectrum; but, as the green, orange, and red rays are unaltered, the two bands of scarlet cruorine are readily seen. The recent menstrual fluid, when dissolved and properly diluted, gives the spectrum of scarlet cruorine; so does urine mixed with menstrual fluid, even if highly bilious.‡

3. Perhaps in icterus, in the typhoid affections, where the blood becomes blackish, in melanosis, characterized by the presence of colored matters in the blood, etc., spectrum analysis might with advantage be applied, suggests M. Benoit.

* A. H. Gallatin: "Note on the Absorption Bands of Hæmatine and Cruorine." (Proc. Lyc. Nat. Hist., N. Y., vol. i., p. 173, 1871.)

† Valentin: "Der Gebrauch des Spektroscops zu physiologischen und ärtzlichen Zwecken," 1863, p. 98.

‡ W. Bird Herapath: *op. cit.*, *loc. cit.*

Table of the Eight Different Blood-Spectra.

1. Red or oxidized cruorine.	5. Hæmine.	The origin of these spectra is unknown.
2. Brown or reduced "	6. Blood, treated by NH_4S.	
3. Brown or oxidized hæmatosine.	7. " acidified ether.	
4. Red or reduced "	8. " HS.	

Notes on the Deviations in Color of Normal and Diseased Blood.

It is red in all the vertebrata; in the invertebrata it is generally not, though some, as the leech, have red blood. In some it is blue, in the snail, for instance. In man, both white and blue blood have been admitted. If drawn during digestion, when much chyle is in circulation, it is whitish. A case of blue blood, the only observation known, is reported by a physician of the Hôtel-Dieu. A woman with a scorbutic affection had hæmorrhage from the nose and eyes, and the napkins were colored blue by the blood. Acids had no effect upon the color, and alkalies destroyed it, properties belonging to Prussian blue. The red color varies: that of woman is said to be less red than that of man, of adults than of children, that of the cat clearer than that of the ox, etc. Pregnancy gives, it is said, a darker tint. There is an organ whence the blood *issues* red—the kidney. Gases cause the most interesting changes of color. M. Bernard says that in very grave typhoid fever he had seen the blood in the veins not rutilant, it is true, but of a cherry-color, so much redder than is natural, that when the vein was opened he had at first supposed it to be arterial.* The venous blood of a bird killed by carbonic oxide is red—not so if sulphuretted hydrogen be the agent. All organs save the kidney can change the color of the blood. It is said oxygen reddens and carbonic acid blackens the blood. When an animal is placed in an atmosphere containing thirty per cent. carbonic acid, it dies, and the blood is found black in all the organs. A red color can come from

* "Notes on M. Bernard's Lectures on the Blood, by Dr. Atlee," 1854, pp. 18, 22.

very noxious gases; thus oxide of carbon reddens the blood very highly. Even after coagulation this red color persists; though the blood is blackened by carbonic acid, it becomes more red on exposure to the air, and arterial blood under the same circumstances darkens. The carbonic oxide prevents any further change, and after two or three days the color of the blood is the same. Sulphuretted hydrogen blackens the blood, and it does not change afterward. This color is of importance in cases of poisoning by charcoal. In these it is not the carbonic acid that kills, for there is never thirty per cent. in the room; it is the carbonic oxide that destroys life, of which one per cent. in the atmosphere is fatal. In cases where persons are killed by charcoal-vapor the blood is found red, except when death has taken place after their removal to another air. If the blood be found black, the person has evidently been removed from the influence of the charcoal before life was extinguished.* The blood-globules are easily oxygenized; this property can be readily arrested by certain substances. Prussic acid, mingled with a liquid in which the phenomena of oxidation are taking place, arrests them immediately. It has exactly the same effect upon the blood, so far as color is concerned, as carbonic oxide and oxygen itself. Given to an animal, the blood is found red, and, as when carbonic oxide is given, a further change in color is prevented. It seems as if the oxidizing property of the blood was at once arrested, the animal dying because it has no oxygen, its blood having lost the property of absorbing it.† When an animal is bled to death from the large veins, the blood at first flows black, but at last it issues red, and there is no distinction between it and that in the arteries. When a limb is detached from the body, with the exception of the artery and vein, or, for greater security, these are cut also, and tubes are used to connect their extremities, the blood for a few moments returns black; but, as soon as the nervous influence is extinguished, it returns red. What proves the necessity of the aid of the nerves, in

* Bernard: *op. cit.*, pp. 20, 21.

† Ibid., p. 21.

the transformation of the blood, is the fact that if the nerves be excited by galvanism, the returning blood becomes black again.* In the blood some substances have their chemical actions entirely masked. When some lactate of iron in solution is dropped into water, and then the cyanide of potassium added, prussic blue is formed. If it is dropped into serum, a kind of combination is produced between the iron and albumen, so that the blue is not formed by the addition of the cyanide. If, however, an energetic acid, that will destroy the albumen, be added, the color will at once be formed.* When the cyanide and the lactate are injected into the jugular vein of a living animal, Prussian blue is never formed in the blood; but, when these substances pass out in the urine, or into the intestines, when the albumen has been destroyed, then Prussian blue makes its appearance.† When exposed to the air, the blood of the cephalopoda becomes a bright blue, and that of crabs becomes blackish.‡

Notes on Quantitative Spectral Analysis of the Blood.

Perhaps before long we will be able to make reliable quantitative determinations with the spectroscope. Thus it might be applied to perfect the method of Welcker for the estimation of the quantity of blood contained in the animal. Suppose that to obtain an absorption-band of a certain width, with a certain light, it is necessary to add to 100 c.c. of water, 2 c.c. of the blood of a dog, for instance. If we rapidly bleed this dog, and then wash with care its circulatory system, we would, by mixing the blood with the washing-water, obtain perhaps ten litres of colored liquid. To refind our absorption-band with its exact width, it would be necessary, for instance, to mix with 100 c.c. of water, 20 c.c. of our liquid. Then 20 c.c. of the liquid would contain 2 c.c. of blood; hence the ten litres would contain one litre.§ Thus might also be estimated,

* Bernard: *op. cit.*, pp. 21, 22. † Ibid., p. 32.

‡ Paul Bert: "Leçons sur la Physiologie Comparée de la Respiration,' etc., 1870, p. 76.

§ Ibid., *op. cit.*, pp. 76, 77.

by this method, the relative quantity of blood-globules or at least of hæmato-crystalline, which the blood of different persons in various conditions contains. Perhaps we might determine in this way the percentage of globules contained in the blood, in chlorosis, etc.

Bibliography of Quantitative Spectral Analysis of the Blood and other Colored Fluids.

W. Preyer: "Annalen der Chemie und Pharmacie," Bd. 140, S. 187–200, 1866.
Berichte der Deutchen Chemischen Gesellschaft zu Berlin, iv., p. 404, 1871.

R. Vierordt: "Ueber die Anwendung des Spectral-Apparates zur quantitativen Bestimmung von Farbstoffen." Berichte der Deutchen Chemischen Gesellschaft zu Berlin, iv., p. 327, 1871.
"Die Anwendung des Spectral-Apparates zur Messung und Vergleichung der Stärke des farbigen Lichtes." 1871.

Hugo Schiff: "Ueber die quantitative Bestimmung von Farbstoffen mittelst des Spektroskops." Berichte der Deutchen Chemischen Gesellschaft zu Berlin, iv., p. 474, 1871.

Additional Notes on the Color of Blood in Animals.

The blood is very rarely colorless: it is so in the echinoderms, the amphioxus, etc. In an immense number of inferior animals it has a feeble bluish color; thus the cephalopoda, the gasteropoda, spiders, etc.; in some a yellowish or reddish tint; as in many insects, crustaceans, etc. These colorations vary not only with the species, but often even with the individual. The annelides generally have red blood, but in some species it is green. In all the vertebrata, except the amphioxus, it presents an intense and well-known red color.* In almost all the invertebrata, the floating corpuscles, the blood-globules, are colorless. But there are some exceptions. Several observers, and especially Rouget,† have found reddish corpuscles

* Paul Bert: *op. cit.*, p. 69.

† Rouget: "Note sur l'existence de Globules colorées chez plusieurs espèces d'animaux." (*Journal de la Physiologie de l'Homme et des Animaux.* Publié par Brown-Séquard; t. ii., p. 660, 1859.)

in several species of Ascidians and in the genus Sipunculus. But, in the great majority of cases, the blood-coloration, when it exists, is due to the plasma in which the corpuscles swim. It is different in the vertebrata where the plasma is almost colorless.

Spectral analysis—that beautiful science founded on the labors of Wollaston and Frauenhofer, of Brewster and Draper, of Bunsen and Kirchhoff—is still in its infancy. We have firm faith that, in the years to come, many difficulties will be overcome by the man of science in the realms of physiology, pathology, and diagnosis, with the new and powerful weapon which they have presented to him.

List of Valuable Works and Memoirs not directly referred to in this Review.

Dr. Hermann Eulenberg: "Die Lehre von den Schädlichen und Giftigen Gasen," pp. 312–314, 1865.

F. Hoppe-Seyler: "Handbuch der Physiologisch- und Pathologisch-chemischen Analyse." 3 Auf. 1870.

"Erkennung der Vergiftung mit Kohlenoxyd." Z. f. annal. Chem., iii., 439. Ph. Mag. (4) xxx., 456.

"Weiteres über die optischen und chemischen Eigenschaften des Blutfarbstoffes." Centr.-Bl. f. d. Med. Wiss. 1864. Nr. 52 und 53.

G. Valentine: "Histiologische und Physiologische Studien." Abth. ix. Zeitschrift für Biologie. Bd. vi.

"Einige neue Beobachtungen, über das Erkennen des Blutes durch das spektroskop." Virch. Arch., xxvi., p. 580.

W. Preyer: "Ueber einige Eigenschaften des Hämoglobins und des methämoglobins." Pflüger: Archiv., Physiolog., i., 395.

"Die Blutkrystalle." 1871.

Kühne: "Lehrbuch der Physiolog. Chemie." 1868.

V. Gorup Besanez: "Anleitung zur Qual. v. Quant. Zoochem. Anal." 1871.

V. Fumouze: "Les Spectres d'Absorption du Sang." 1870.

Huizinga: "Chemisch.-biologische Notizen über Ozon." *Centralblatt*, 1867, p. 323.

Koschlakoff et Popoff: *Centralblatt*, 1867, p. 405.

Koschlakoff et Bogomoloff: *Centralblatt*, 1868, pp. 609 et 627.

Nawrochi: *Centralblatt für die Med. Wiss.*, 1867, p. 178.

L. Popoff: *Centralblatt für die Med. Wiss.*, 1868, p. 657.

HOPPE-SEYLER: "Jahresbericht von Virchow und Hirsch," 1868, p. 89. "Ueber die Zersetzungsprodukte des Hæmoglobins," Berichte der Chemischen Gesellschaft zu Berlin, 1870, p. 229. (Discovery of the "substance hæmochromogene." Hæmatine does not seem to be the direct product of the decomposition of hæmoglobine. There is an intermediate body, called by Hoppe-Seyler "substance hæmochromogene." The presence of oxygen is necessary to the formation of hæmatine at the expense of the hæmoglobine. If the decomposition of this latter is effected in a vacuum, it is no longer hæmatine which is formed, but a very unstable body which is transformed into hæmatine as soon as it comes in contact with oxygen.)

H. FREY: "Traité d'Histologie et d'Histochimie." Traduit de l'Allemand sur la 3ième édition, par le Dr. P. Spillman. Avec des Notes et un Appendice sur la Spectroscopie du Sang, par le Dr. Ranvier. 1871, p. 777.

EULENBERG et VOHL: "Archives de Virchow," t. cxlii., p. 161.

The Academy then adjourned.

STATED MEETING, OCTOBER 19, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DRS. C. M. BELL, J. ACKERMAN COLES, EDWARD J. HOGAN, JOSIAH C. NOTT, CHARLES P. RUSSELL, and JAMES V. S. WOOLLEY, were inaugurated Resident Fellows.

Dr. CHARLES P. RUSSELL then read the paper of the evening, entitled—

VITAL STATISTICS AND THEIR CLAIMS UPON THE PROFESSION.

BY CHARLES P. RUSSELL, M. D.

MR. PRESIDENT AND FELLOWS OF THE ACADEMY:

IN the paper which I shall have the honor of reading before you this evening, I have attempted little originality. The subject of Nosology is an old one. Its application to statistical research, on a large scale, is of more recent date. Both have been expatiated upon by many eminent men to whom I shall occasionally refer, and of whose language I shall frequently avail myself as the best adapted to illustrate the theme.

Permit me in the beginning to sketch briefly the origin and progress of those efforts toward nosological classification and generalization, whose fruition we are now enjoying. The vast throngs of figures relating to life and to death, which within a few years have accumulated by such means, are slowly but certainly unfolding the mysterious laws of national and cosmical disease. We may liken them to the myriads of stars upon whose countless array midnight philosophers have looked, and read to the world the majestic laws of space.

We know that in antiquity the study of medicine was monopolized by the priestly caste. The Hindoos appear to have been the earliest to turn their attention to this subject; as one of their sacerdotal medical treatises is referred to an age fourteen centuries before Christ, or nine hundred years anterior to Hippocrates. This was called the Ayur Veda, and was a synopsis by professional writers of still older medical lore. It contained a rude classification of diseases whose principal divisions were: Psychological disorders, founded upon the Eastern superstition of demoniacal possession; infantile complaints; and affections of the generative functions. It treated also of surgery, pathology, toxicology, materia medica, and hygiene. Other works of a similar character are mentioned as having been extant among the Assyrians and the early Egyptians and Greeks. Although some endeavor to arrange, describe and individualize, diseases may thus be traced to a period far anterior to the age of Hippocrates; the descriptions handed down to us are exceedingly imperfect. In ancient times, as in our own, pestilences naturally attracted most of the popular attention, on account of the consternation created by their ravages, and they have therefore frequently received historical mention. But neither the classic historians of the Jews, the Greeks, nor the Romans, described with any precision the plagues which swept over ancient countries. Notwithstanding that Thucydides gave an elaborate description of a pestilence by which Athens was devastated, its nature remains a mystery, although it was probably an epidemic of small-pox. A great number of plagues were mentioned by Livy, but it is

impossible to determine their character. Hippocrates and Galen were more precise in their accounts of epidemics and other affections, but still confounded many and failed in accurately defining others which they have recognized and understood. Celsus, who flourished during the reign of Augustus, left a remarkable and systematic review of the maladies then prevalent. Notwithstanding the confusion of diseases and ambiguity of definition which characterized the writings of these distinguished physicians, their delineations were often so perfect as to serve the purposes of modern observers. Although they were frequently erroneous in their speculations, they were clear and perspicuous in their narrations of pathognomonic facts, and thus establish a basis for nosological arrangement. But the analysis of diseases, like the other departments of medicine, was a slowly-progressive science, interrupted and modified by political events. During the middle ages medicine participated with its sister arts in the general obscurity and barbarism. Its scientific cultivation was confined to the Saracenic Schools—the principal of which flourished at Bagdad, Cairo, Cordova, and Fez. The renowned Arabian physicians Avicenna, Avenzoar, Theophail, and Averroes, were familiar with the aphorisms of Hippocrates and the commentaries of Galen. It appears that on the conquest of Alexandria some books were saved from the burning of its magnificent library, and that among them were those mentioned, which were soon translated by the Arabians. Thus they became disciples of the Greek pathology, resolving diseases into the four great classes, whose peculiarities were severally attributed to the sanguineous, the bilious, the phlegmatic, and the melancholic temperament. Many of their religious dogmas were necessarily interwoven with the doctrines which they promulgated.

Although systems of authentic necrology have been in vogue for three hundred years in some places—for example, in Geneva, Switzerland, where all deaths within that period have been recorded—it was considerably later that the endeavor was made to precisely define and methodically classify diseases. "Plater," in the language of Mason Good, "may

be regarded as the morning-star that first glimmered in the hemisphere of symptomatology, as Servetus was in that of the circulation of the blood." The light of both was feeble and tremulous, but it twinkled in the midst of darkness and led on to the brightness of day. His work, entitled "Praxis Medica," in which he gives an imperfect sketch of a plan of symptomatic nosology, was published in 1602.

Sydenham subsequently seized the thread which Plater had first taken up, and in 1675 suggested giving to diseases short definitions, such as those by which plants were distinguished. His recommendation was not reduced to practice until 1731, when Franciscus Boissier de Sauvages, a celebrated professor of medicine at Montpellier, published an outline of a nosological system after the model of that employed by botanists in the classification of plants—by classes, orders, genera, and species. Linnæus was at this period carrying forward his remarkable system of botany. Sauvages's treatise, which had previously been strongly indorsed by Boerhaave, was entitled "Nouvelles Classes de Maladies." He improved it from time to time until, in 1768, shortly after his death, appeared the culmination of his labors in his renowned work, the "Nosologia Methodica." This contained the first full and systematic arrangement of diseases ever executed. Sydenham's suggestions had quickened in the well-stored and colossal brain of Sauvages until thence sprang forth the wisdom-armed creation that gave form and substance to every successive endeavor of the kind. He was fond of acknowledging his indebtedness to Sydenham, whom he styled "Anglus Hippocrates." Sauvages's Nosology embraced ten classes—each introduced by an elaborate pathological synopsis—44 orders, 315 genera, 2,500 species, and innumerable varieties, with the definition of each, its synonyms, history, diagnosis, prognosis, and treatment. This almost endless category called forth from M. de Ratte, in his eulogy of Sauvages, delivered before the Royal Society of Sciences at Montpellier, the exclamation, "Quel nombre prodigieux d'ennemis!"

The distinctions of Sauvages were generally founded upon

symptomatic characters. But his system was too diffuse, too full of ramifications—a nosological labyrinth, immense, wonderful, and perplexing. His herculean labors, however, produed a deep impression upon the scientific world, and, like all great inventions, his production was soon subjected to improvements by other master-minds throughout Europe. Hence, the subsequent attempts of Linnæus, at Upsala, in his "Genera Morborum;" of Vogel, at Göttingen, in his "Definitiones Generum Morborum;" of Sagar, at Iglaw, Moravia, in his "Systema Morborum Systematica;" and of Cullen, at Edinburgh, in his "Synopsis Nosologiæ Medicæ." That of Cullen appeared in 1792, and, on account of its intrinsic merits, at once supplanted all others. His method of classification resembled Sauvages's somewhat in form, but was even more comprehensive, while it was characterized by greater brevity, simplicity, and accuracy of definition. It contained but four classes—Pyrexiæ, Neuroses, Cachexiæ, and Locales. He reduced the 44 orders of Sauvages to 20, and his 315 genera to 151. He did not attempt to arrange the diseases according to their proximate causes, but more especially with reference to the pathological elements upon which they depend. Such a system, however, must necessarily have partaken of the imperfections which marked contemporaneous opinion or conjecture, and have been subjected to continual modification. Naturally, therefore, it became gradually overwhelmed, and sank at last under the waves of increasing knowledge.

Cullen's chief competitors were Selle, Ploucquet, Pinel, Crichton, and Darwin. Time does not admit of more than an allusion to them. They made few innovations upon Sauvages's system, generally recognizing the same principle of making divisions dependent upon the prominence of particular symptoms.

The first attempt made in Great Britain to improve on Cullen was that of Macbride, whose four primary divisions were into Universal, Local, Sexual, and Infantile disorders.

More modern nosologies have been devised by Parr, Bichat, Vicq-d'Azyr, Richerand, Young, Mason Good, and Farr.

Young and Good classified diseases according to the special functions involved. Mason Good's division was into affections of the Digestive, the Respiratory, the Sanguineous, the Nervous, the Sexual, and the Excernent Functions, with a distinct class for fortuitous lesions and deformities.

Prof. Carswell proposed a classification founded upon the elementary phenomena and products of diseases—a rather impracticable one in the present state of medical observation.

Many other nosological classifications have been propounded and employed by eminent men to facilitate their own special investigations. Some are suitable only for the practitioner; others for the anatomist, the physiologist, the pathologist, the medical jurist. All such endeavors to reduce to system have helped to increase our stock of knowledge in fulfilling the specific objects for which they were devised. In classifying diseases, the extent to which their analysis should be carried must depend upon circumstances, and for statistical uses the individuality of the facts should be sufficiently preserved, while the distinctions should not be more subtle than is necessary to separate the facts. Such a system, and one applicable to the wants of different populations, was a problem first solved by Dr. William Farr. A statistical congress was convened in Paris, under the auspices of the French Government, in September, 1855, to consider this subject, and they agreed upon a nomenclature of the causes of death essentially the same as that proposed by Dr. Farr. At another congress, held in Vienna in 1857, a uniform nomenclature and plan of registration for all the European states was determined upon. Dr. Farr's classification was not so generally adopted; but has since been making its way in Germany and other portions of Europe. This nosological classification, though by no means perfect, doubtless possesses, in its practical bearings upon public health, advantages over every system that has preceded it. Let us examine for a few moments the principles upon which it depends. Its primary divisions are five, and are founded upon the manner in which diseases of similar character or type affect the population. Thus, we have, first, the great

class of zymotic diseases, chiefly comprising fevers *par excellence*—the epidemic, endemic, and contagious or infectious disorders—which suddenly attack masses of people—which spring from different sorts of malaria or from specific communicable poisons—contaminate the atmosphere and the water, and decimate, in a brief time, civil and military communities. We read in sacred history of whole armies having been suddenly swept away, as that of Sennacherib, which, while besieging Jerusalem, lost 185,000 men in a single night under the deadly breath of the destroying angel— a beautiful metaphor, probably, for the swift and invisible blow of the pestilence. It has been well remarked that these diseases "distinguish one country from another—one year from another." They have formed epochs in chronology, and, as Niebuhr has shown, "have influenced not only the fall of cities, such as Athens and Florence, but of empires." This great class of maladies is the index of salubrity; it is this class which varies to the greatest extent in different climates and seasons; that constitutes the principal difference between the health of various peoples and periods. It is eminently proper, therefore, notwithstanding the features which serve to distinguish these affections clearly from one another as from those of a different class, that they should be grouped together. The resemblance in their effects and influence is of vastly more importance than the disparity in their several traits. Associated in the same class, and partaking of their peculiarities in a minor degree, we have epizoötic, parasitic, and inoculated diseases, with those resulting from the deprivation of sustenance and abuse of drink.

Besides the zymotic disorders there is, as Dr. Farr expresses it, "a legion of diseases, never halting, and not so much controlled by external circumstances, viz., sporadic diseases, or ordinary maladies of every-day occurrence." These are sometimes hereditary, sometimes apparently spontaneous, or traceable to various exciting causes; but not generally regarded as capable of direct propagation. They participate frequently in the fluctuations of zymotic diseases, being often modified

thereby, and thence assuming different types. First among these are the constitutional diseases, including the diathetic, as rheumatism and cancer, and the tubercular, as phthisis, scrofula, marasmus, and hydrocephalus. Next we have the great class of local disorders—affections of special organs grouped in distinct systems, as meningitis in the nervous; aneurism in the circulatory; pneumonia in the respiratory; peritonitis in the digestive; Bright's disease in the urinary; metritis in the generative; necrosis in the locomotory and osseous; and boils in the integumentary.

Next come developmental diseases, or those arising from morbid action of the formative, reproductive, or nutritive processes, as disorders of newly-born children; premature and preternatural births; cyanosis; atelectasis; spina bifida and other malformations; diseases of women incident to the puerperal state or to menstrual irregularity; affections peculiar to advanced age, such as senile gangrene and decay; and disease consequent upon imperfect nutrition, as atrophy.

The final class embraces deaths due to violent causes, the direct results of physical or chemical forces, whether applied accidentally, suicidally, or homicidally, and comprising, as a rule, cases necessarily subject to judicial investigation. Dr. Farr remarks that, in a political point of view, violent deaths are of great importance, as they bear more upon the efficient part of the population. He regards them as a very prominent class, which should be distinguished from all the others, and this illustrates the point: "A watch or machine may be destroyed by dashing it upon the ground or throwing it into the fire. Its motion may also be arrested by internal causes, having their source in its own mechanism. In the body are innumerable processes going on which are not mechanical, and a change in which will destroy life, as the acid fermentation destroys milk or wine without any mechanical agency."

I have taken the liberty to review Farr's classification at some length, and have consequently reiterated points which are undoubtedly familiar to most of those present. There may be others, however, who may not have had occasion to exam-

ine this nosology, and therefore I have indicated its salient features, in order that my subsequent remarks may be correctly interpreted by all.

Before proceeding to detail the difficulties which attend the proper classification of the various causes of death as they are returned to the Health Department, I will say a few words with reference to the history of our municipal registration.

The official memorial of deaths in New York City dates back to 1798, in which year were registered the names of five persons, three infants and two adults. It is somewhat singular that both of the latter were negroes. In 1799 and 1800 no names were entered on the register. In 1801 there were 43; in 1802, 843; in 1803, 1,134; and in 1804, 725. From that date up to 1811 none were registered save in 1808, and then but 424. In 1811 only 40 names were recorded. Thus, within fourteen years, in an average population of 70,000, there were registered an annual average of but 230 deaths. In 1812 there were recorded 2,503 deaths—the first fair approximation to the actual number.

Many of these early entries were indefinite, as, for example: "Mrs. Dick's child, aged two years; place of death Greenwich Street"—no disease specified; and "George Bennison's three children, interred in one grave." There were a few registries of the names of persons dying elsewhere than in the city, as in Westchester or even Philadelphia—one being simply recorded as "brought seventy miles for interment" here. Under the heading of "remarks" in these venerable books, is occasionally found some quaint record. As, for example, we are informed that Wm. Powell died on August 1, 1803, of a "casualty; by taking too large a vomit." Fortunately, perhaps, for the posthumous reputation of Mr. Powell's physician, there was preserved in those days no record of the names of medical attendants.

Although individual observers have left us accounts of various diseases which prevailed here in the last century—principally those of a pestilential nature—no attempt was made to collect authentic statistics of general mortality until

1803, when John Pintard, Esq., published the first bill of mortality, extending from November 1, 1801, to January 1, 1803. Mr. Pintard at the same time endeavored to direct public attention to the importance of a complete registration, not only of deaths, but also of births and marriages; but without much success, as no laws encouraging such registration were enacted by the State until 1847, and it was only in 1853 that a statute embodying stringent and specific provisions in these regards was adopted. But very little attention, comparatively, was paid to the act, and no attempt made to enforce its requirements. The number of marriages and births returned increased, to be sure, but were still exceedingly deficient, as they remain to this day, not much to the credit of those who are delinquent in contributing to such important branches of registration. The number of deaths reported was also far from being complete. The City Inspector supplied undertakers with burial-permits signed in blank, and they or the cemetery-keepers returned the certificates of death at their discretion. The most flagrant abuses were thus possible. This practice continued to a certain extent up to the organization of the Metropolitan Board of Health, in 1866, after which, no burials were permitted until the proper certificate of death had been deposited at the Central Office. Imperfect as had been the registration of death prior to 1866, the classification of their causes had been even more so. As there was no supervision of any description over the returns, a large number of certificates were necessarily received which assigned the most indefinite and unmeaning causes, such as congestion, corpulency, hereditary or congenital disease, extravasation, inflammation, inflammatory fever, polypus, ulceration, bleeding, etc. It seems almost incredible that, but a little over five years since, such certificates should have been sent forward to serve as a basis for most vital scientific inquiries. Very little had been attempted at classifying even those diseases which were recognizable until 1843, when Dr. Griscom, during a brief term of office in the City Inspector's department, introduced the first improvements. Thereafter, the principal zymotic affections were

grouped together; but many others were assigned an uncertain and variable position. This was a great step in advance, but was abandoned in 1859 by Dr. Ramsay, then registrar of vital statistics, who returned substantially to the old method of simply alphabetical arrangement. He made a separate table of deaths from external causes, which, besides those from violence, embraced cyanosis, malformation, old age, premature births, and sunstroke; a rather motley assemblage.

In March, 1866, the newly-organized Metropolitan Board of Health intrusted the vital statistics of this city to the supervision of Dr. Elisha Harris, to whom we are indebted for the adoption of Farr's system of classification. Since that time the causes of death have been arranged in accordance with that method—those of each week being separately classified—the weeks being consolidated into quarters and the latter into years. As the same system is pursued in Great Britain and other countries, constant international exchanges and comparisons are practicable, which are not simply confined to individual affections, but are applicable as well to immense groups of cognate diseases. In this manner, statistics of mortality assume vast importance, and present for our consideration manifold questions of a physical, political, and social character. They determine the laws which regulate the duration of life, and afford bases for calculations materially affecting the interests of millions. Statistics are far from being the barren array of figures ingeniously and laboriously combined into columns and tables, which many persons are apt to consider them. They constitute, rather, the ledger of the people, in which, as the merchant in his books, the citizen can read at once all the results of a year or of a period of years as compared with other years or periods, and can deduce the profit or the loss which has accrued to the account of morals, vitality, education, wealth, or power. It has been well said that "science has nothing to offer more inviting in speculation, than the laws of vitality, the variations of those laws in the two sexes at different ages, and the influence of civilization, occupation, locality, seasons, and other physical agencies, either in generating diseases or in improving the public health."

But, putting aside this broad and philosophic view of the importance of mortuary statistics, it is obvious that the appli cation of their deductions must be of great benefit to the physician as a practitioner. This was perceived even as far back as Sydenham, who inculcated the doctrine that the treatment of diseases should have a reference not only to the immediate symptoms and to the season, but also to the epidemic constitution of the year and the locality. It has been said by a distinguished author that "man is not born, does not live, does not suffer, does not die in the same manner on all points of the earth. Birth, life, disease, and death, all change with the climate and soil—all are modified by race and nationality." Medicine, with the other natural sciences, has now abandoned vague hypothesis for truths determined by observation. Numerical expressions are now substituted for uncertain and conjectural assertions. Only a limited number of facts, however, are contained within the horizon of a single observer. Power is derived from the aggregate observations of many. But, however remarkable may be the circumstances which are thus brought into view, they must owe all their value to proper classification, or, in other words, to the manner in which they are presented, both relatively and as a whole. The significance of mortuary classification is entirely dependent upon the accuracy of information supplied by death-certificates. As this becomes more apparent to the profession, and they recognize the full bearing upon the science of medicine of such masses of facts, they must also perceive that every single contribution to vital statistics is, in its ultimate value, as so much "bread cast upon the waters" destined to be "returned after many days."

These certificates of death pour into the Bureau of Vital Statistics incessantly, in a huge, chaotic mass, without form or meaning; they must at once be severally grouped, analyzed, and arranged in their appropriate places. Their resultant figures must not lie. The whole edifice must be built upon a substantial foundation of truth, or it will be defective, however massive and imposing in appearance. But, in the first

place, gentlemen (and in this lies the inherent responsibility of the profession itself), the fundamental facts of which I have spoken *must be facts*—clear and unmistakable; and each must be so unequivocally presented as to admit of but one interpretation. These facts are communicated to us only upon certificates of death; they should not be couched in vague or ambiguous language, for upon a proper conception of their meaning depends the whole fabric which we construct.

To many it may seem superfluous to dwell upon this point; but it is a vital one, and moreover one scarcely appreciated by the profession. Dr. Samuel Henry Dickson, of Philadelphia, in an article recently published in the *Medical Record*, and having reference to the same point, says: "My purpose in preparing this paper was to urge upon my medical brethren the necessity of taking action upon the subject discussed, and rescuing the profession from the shame of a registration whose defects are all owing to their neglect, and whose value to the community would be vastly enhanced if they would aid the officials in the performance of the duties which they endeavor to fulfill, but which are rendered so much more difficult by the want of skillful and prudent direction." In this connection, however, I am happy to state that the profession of New York, as a rule, are now much more careful and minute in their returns than formerly; such indefinite causes as cancer, congestion, dropsy, inflammation, fever, hæmorrhage, etc., of which Dr. Dickson justly complains, being here rarely given. It may be safely asserted that our statistics of mortality are now more accurate than those of any other city of the United States. But there are a few important points occasionally misunderstood or neglected by physicians, to which I shall refer, as I am confident that they do not receive the attention which they deserve.

The functions of the Bureau of Vital Statistics, so far as they relate to mortality, are threefold and distinct. They consist—first, in authorizing the legal interment of the dead; second, in the accurate registration of various particulars descriptive of each deceased for future identification; and,

third, in the scientific and systematic classification and analysis for public use of those affections which terminate human existence. Now, as the law makes it incumbent upon every physician who has attended a person in a last illness, to present to the Bureau of Vital Statistics, within a specified time, a certificate of death containing various details; and as such certificates afford the only data upon which the bureau is enabled to accomplish the functions stated, it is essential that each certificate should be distinctly legible, and convey, in brief, as exact a statement as the physician can possibly supply. The certificate upon presentation is first critically examined, to discover whether the death can be attributed to any cause which will necessitate a coroner's investigation. In such an instance, it is the legal duty of the physician, or the friends of the deceased, to report the death directly to the coroners or the police; but, as neither the general public nor the profession are fully acquainted with the provisions of the law, many of these cases are submitted to the judgment of the registrar. Section first of the amended law upon this subject, passed at the last session of the Legislature, reads as follows: "Hereafter, when, in the city and county of New York, any person shall die from criminal violence, or by a casualty, or suddenly when in apparent health, or when unattended by a physician, or in any suspicious or unusual manner," the coroner shall be summoned to hold an inquest. Laws to determine the necessity of inquests have always been extremely vague and difficult of application. The act of 1847 directed the coroners to investigate the death of every person dying "*under such circumstances as to require an inquest*," which according to the revised statutes was to be held whenever "*a person had been slain or had suddenly died.*"

The amended law which I have quoted above seems to be sufficiently explicit. In order that it may not be violated, it becomes necessary for the physician in every instance to state clearly the actual character of the fatal affection, and particularly the nature of any external cause in diseases which so often depend upon violence; such as fractures, wounds, ery-

sipelas, peritonitis, gangrene, pyæmia, tetanus, phlebitis, gastritis, etc. If such affection have not arisen from any such cause, its character should be specifically indicated; as, if the registrar be not satisfied in this respect, the certificate is necessarily returned for the required information before the burial can be authorized.

The coroners, however, do not insist upon the literal interpretation of the law cited, but leave much to the discretion of the registrar. For instance, it may happen that in an old chronic case, as one of phthisis, bronchitis, Bright's disease, senile decay, and the like, the physician may not have visited his patient for weeks or not even months before death, and yet may be morally certain of the cause. In similar instances, as well as most cases of apoplexy, heart-disease, and trivial accidents, the coroner is not notified, although by a strict construction of the law he might claim an inquest.

2. Each item of name, age, condition, etc., should be obtained from the friends with scrupulous care, and should be inscribed upon the certificate so distinctly as to render a mistake impossible. All these particulars have to be copied into registers; transcripts from such records are constantly demanded for legal purposes, particularly for transmission to Europe, where in most courts of law they are the only recognized documents for special uses, and become invalidated by the slightest error. Their inaccuracy, therefore, often occasions indefinite trouble and prolonged litigation. Almost daily mistakes of this sort, originating with the physicians, are discovered, and their amendment is frequently attended with great difficulties.

3. With regard to causes of death upon certificates, I shall offer a few additional suggestions.

The question is frequently put, "What is meant by first and second causes of death?" The terms are certainly not exact, and are apt sometimes to occasion confusion. As a general rule, however, it is immaterial which cause is placed first, provided we can comprehend perfectly which was the predominant or original agent to which the fatal result may be

imputed. For example, a child has died from scarlatina, with the so-called diphtheria; it is not material which of the two be written as the first cause; it will be registered as a case of scarlatina. Measles or whooping-cough will be considered as the cause, although bronchitis, pneumonia, or meningitis, may have been the immediate factor. A death will be ascribed to typhoid fever, although perforation and peritonitis may have actually carried off the patient. Cancer will be credited with the fatal result, although it may have occasioned any imaginable complication. Phthisis will be selected, although pneumonia may have accelerated the death.

In some cases it may be advisable for the physician not merely to state the causes of death, but to add a few words of explanation. A person may have suffered from syphilis, and may have died of acute laryngitis without the syphilis having had any perceptible influence in producing the laryngitis, although possibly complicating it; or, on the other hand, the laryngitis may have been unquestionably the consequence of syphilis. In the former case laryngitis would be chosen by us as the cause of death, in the latter case syphilis; while it is obvious that, whether one or the other were placed as the first or as the second cause without further explanation, an error might be committed by the registrar.

When two distinct diseases exist at one and the same time, as, for example, small-pox and phthisis, it is essential that the one actually causing death should be specially distinguished. The same is true of many affections dependent or not dependent upon other coexisting disorders, as Bright's disease and pericarditis or pleuritis, phthisis and diarrhœa, rheumatism and heart-disease.

It is also important that the duration of each cause of death should be specified. A miscarriage may have left in its train a metritis, or a metritis may have been the only attributable cause of a miscarriage; the length of time that each has existed before death will afford a clew to the proper cause. Diarrhœa and marasmus may be assigned as the two causes of death, and are perhaps more frequently returned together than

any other two diseases; either may provoke and induce the other, and it is only from the duration of each that we can determine which to select.

It might be asked, "Why, if confusion is liable to arise from want of clearness in defining the two causes, would it not be better to state simply one cause?" We answer that the statistics of particular affections are often as interesting and as valuable as those of groups of diseases, and the different types and peculiarities by which certain diseases are distinguished at various periods are indicated, to a great extent, by the fluctuations of their complicating disorders.

There are a number of diseases, a list of which is printed upon the backs of the present certificates of death, concerning which certain information is requested, either for statistical purposes or to insure correct classification. I will mention some of them. Cerebro-spinal meningitis is quite commonly given as the direct and only cause of death. All of these cases were formerly assumed in the Bureau of Vital Statistics to be those of the malignant purpuric or spotted fever, and they were, therefore, all classed with the zymotic diseases. Being struck with their large number, I have been led to make a special inquiry into the character of several cases, none of which were found to have been of the epidemic variety, but, simple inflammations, such as the name would imply. It is therefore necessary to discriminate between the two kinds, one of which belongs to the zymotic class, and the other to the order of nervous affections. Enteritis and gastro-enteritis may be diarrhœal and zymotic; or, on the other hand, due entirely to local excitement. Erysipelas may apparently arise spontaneously, or may be superinduced by traumatic causes, when it would occupy a different position in our list. Gangrene may be of the specific hospital variety (zymotic); may be produced by embolism (local); may be of the senile description (developmental); or may be the consequence of external violence. Gastritis may be occasioned by internal causes (local); or may be the immediate result of intemperance (zymotic); or may be excited by accidental, suicidal, or homicidal applica-

tions from without (violence). Mania and convulsions may be of the peculiar kind dependent upon the puerperal condition (developmental); or may be more directly referable to cerebral disorder (nervous).

A miscarriage or abortion may be the result of relapsing fever, syphilis, external violence, etc. Peritonitis may be puerperal or spontaneous, or occasioned by some other disorder, as volvulus, or may be traumatic. Phlebitis may depend upon a wound or upon a surgical operation, as that for varices may be of the peculiar variety known as phlegmasia dolens, may arise without assignable cause, or may terminate some other affection. The same is true of pyæmia, and either of these two diseases is frequently the *only* cause of death returned in cases of *puerperal fever.* Tetanus may be idiopathic or traumatic, or the trismus of new-born children.

I have selected a few of the most prominent diseases to illustrate the positive necessity of each physician's being sufficiently explicit in defining a cause of death to enable the registrar to classify it properly.

If a woman die from puerperal peritonitis or puerperal convulsions, it will not suffice if we are informed simply that peritonitis or uræmic convulsions was the cause: we must know more, or our statistics will be impaired in value from diseases having been assigned false positions.

There are a few additional points (not absolutely necessary for purposes of classification), but which should be made as explicit as possible upon certificates, in order to afford valuable statistical information. In cases of abscess, ulcers, aneurism, necrosis, caries, cancer, etc., the location of the disease should be indicated. Heart-diseases, if an accurate diagnosis have been made, should be distinguished, as whether pericarditis, endocarditis, fatty degeneration, valvular disease, etc. The special variety of hernia should be stated. Malignant pustule —can it be referred to epizoötic contagion? Premature birth —the cause and fœtal age, if ascertainable. Preternatural birth (by which we understand one followed by speedy death after prolonged labor, instrumental and manual delivery, etc.)

should be described. In cases of syphilis, the variety, chief location, and manner of death, should be given. Every surgical operation with fatal result should be specified, as well as the affection which necessitated it.

The above information need seldom require more than half a dozen words. But, if a little more extended statement be necessary in order to give a perfect idea of the cause of death, it is assuredly better that the physician should devote a few moments to such an explanation rather than be misunderstood. Every day we receive one or more certificates with these brief but satisfactory indorsements.

There are some terms employed not infrequently whose vagueness is very objectionable—as, for example, *febris nervosa*, standing at one time for typhus or typhoid fever, and at another for something else; and *gastric fever*, which in one instance may signify remittent fever, in another typhoid, or nothing but gastritis. It is inexpedient to abandon, as Aitken remarks, the universally recognized names of distinct diseases, or to substitute new ones in their place. Sauvages and Cullen insisted particularly upon this point. "Words," says the former, "are good only with respect to their signification." However clear may be a person's ideas within himself, he can communicate them to others only by expressions equally clear and possessing a definite meaning.

It has been maintained by some nosologists that it is possible to eliminate entirely from our nomenclature all expressions of acknowledged indefiniteness. They would suppress the use of the terms *inanition, marasmus, convulsions, and old age, or senile decay.* It seems to me, however (although no one more fully realizes the importance of accurate registration), that it would be impossible, with our present appliances and means of diagnosis, to avoid the employment of such expressions. Vague though they may be, they yet represent ordinary conditions, often of obscure origin, whose elucidation would be impracticable. They are, besides, universally employed, and their statistics are therefore useful for comparison.

There are a few questions connected with this subject of

nomenclature which seem proper for discussion and consideration in the Academy. In Dr. Dickson's article, before referred to, he takes occasion to criticise the term *cholera infantum* as a trivial and unmeaning one on account of its not specifically designating any separate disease, and, even if it did, as being mostly impossible of diagnosis. I have for some time entertained a similar view, and have not admitted the name into our statistical tables and reports during the present year, believing that it should be abolished and all the common diarrhœal complaints of children classified simply as *diarrhœa.* Our daily experience teaches us that the certificates in these cases are entirely useless for any separate statistical application; for the numerous expressions *summer complaint*, *cholera infantum*, *diarrhœa*, *enteritis*, *entero-colitis*, *gastro-enteritis*, *intestinal catarrh*, *gastro-intestinal catarrh*, and *muco-enteritis*, are employed indiscriminately. The word *dysentery* is also occasionally used in cases of the same character where no dysentery has been present; but such instances are too rare to affect the statistics of that disease materially. According to our returns, *cholera infantum* is a disease of all seasons, of duration from a few hours to many months, and of occurrence in children up to ten years of age, as we learn from the Report of the Metropolitan Board of Health for 1869.

Typho-malarial fever is an expression lately become fashionable. When used to signify the asthenic form of *remittent* or *intermittent*, it would be more to the purpose, I think, to substitute those terms. A few of such cases, however, have, as I am informed, been examples of *continued fever*, dependent upon marsh-miasm, non-contagious, and devoid of the lesions characteristic of *true typhoid.* In others there has been a suspected but not demonstrated coexistence of the two specific poisons. The designation *typho-malarial* would seem a very appropriate and significant one for the last two varieties.

Diphtheria and croup appear to be often confounded, or rather either the former term or that of diphtheritic croup is quite commonly employed in place of the latter. During

1870 there were reported 241 fatal cases of diphtheria in children under five years old against 381 of membranous croup. I believe that the general experience of the profession for that year should exhibit a much smaller proportion of cases of genuine diphtheria, even if its relative fatality be greater.

This paper has thus far been confined exclusively to the consideration of nosology, the most important element of vital statistics. Man, however, is ushered into existence under natural circumstances almost as impressive as those which circumscribe his duration of life, and which attend its surrender. While millions are divesting their being of earthly garb and entering upon their eternal inheritance, other and greater millions are assuming the heritage of life in forms moulded by chance and stamped with ancestral peculiarities. If, therefore, it be profoundly interesting to contemplate the multitude of agencies which impel this innumerable caravan of pilgrims toward their destination, it is almost equally instructive to analyze the manifold causes which have contributed to their assembling together.

The registration of births in New York has always been shamefully deficient, and, being elsewhere often assumed to represent fact, has been a frequent source of odium against our city. In 1854, the year following the adoption of a stringent law upon this subject, 17,076births were recorded ; but, as only a voluntary compliance with the law was found to be required, the returns gradually fell off until, in 1865, they became reduced to about 5,000. During the ensuing year, which was marked by the establishment of the Metropolitan Board of Health, they reached 10,000, and have since been slowly augmenting. Still, in the past year only 14,524 births were returned against 27,175 deaths. Compare these figures with those of the city of London for the same year, in which there were registered 77,278 deaths and 113,499 births, a birthrate of 35 to the 1,000 inhabitants, while our statistics exhibit but 14 to the 1,000. Truly we would seem a degenerate race !

This culpable failure to return births entails a vast amount of trouble and even suffering upon innocent people. As I

have previously mentioned, certified transcripts from the public records are the only documents recognized as proofs in certain cases by most European courts of law. Identity, legitimacy, inheritance, daily depend upon such proofs. In some portions of Germany a child is entirely debarred from admission to the public schools unless his parents furnish this record of his birth. But a few weeks since I received a very eloquent appeal from a gentleman in Hamburg, whose child had been born here in 1867, without the medical attendant having taken the trouble to return the birth. The writer complained bitterly of the doctor's negligence, and stated that he was unable to place his child at school without such authentic proof of its birth. The physician has now lost all remembrance of the case, and it will be extremely difficult to remedy the wrong he inflicted. The act requires both the parents, and the medical attendant, to furnish these returns. Many of the parents, however, are ignorant or illiterate, and a large majority are foreigners, unacquainted with the law or unfamiliar with the language. During the past year there were recorded the names of 9,282 children of purely foreign extraction, and only 2,553 of native parentage. These peculiarities of our population, as distinguished from more homogeneous ones, as well as the somewhat indulgent administration of our laws, would render ineffectual at present any measures to compel parents generally to report their children's births. That duty, therefore, devolves upon physicians; and in accomplishing it they must not deem themselves disinterested; for thereby they are advancing the interests not only of their patients, but also of their own profession in supplying particulars which, when massed into statistics, become of acknowledged importance to medical science. The disparity in the sexes born at different periods, the average number of women bearing twins, triplets, etc., the proportion of offspring from native or foreign progenitors, the ages and occupations of fathers, the average number of children born at different periods of female life and in different seasons, the influence upon reproduction of the relative ages of parents—these and other kindred ques-

tions particularly concern the medical man, and the source of their solution lies in the large number of accumulated facts which constitute these statistics.

Moreover, the actual number of births occurring in a given community each year is indispensable, in conjunction with other factors, for computing the increment of population during years intervening between those of official enumerations —and consequently, for the determination of the true death-rate. The remarkable precision with which this increase may be approximated is shown by the London tables, according to which the estimated population of that city on April 2, 1871, was 3,247,631; while the decennial census, taken on the same night, gave a population of 3,251,804—a difference of only about 4,000 in three and a quarter millions—one almost inappreciable.

The limits of this paper do not permit me to dwell longer upon this subject. I trust that sufficient has been said to persuade those physicians who refuse or neglect to return their births, that in so doing they are violating a moral, a legal, and a professional duty.

And, in conclusion, I ask you, gentlemen, if it is not time that bodies of intelligent and public-spirited physicians, such as this Academy of Medicine, should take cognizance of, and condemn so reprehensible a state of things, for which doctors are principally responsible, and which, in an age like the present, is a reproach upon a civilized community? Its reform concerns you, not only as representatives of a noble profession, for whose advancement all stand ready to sacrifice personal consideration, but also as a body of conscientious citizens, bound by every obligation to contribute whatever is equitable toward the success and reputation of the commonwealth.

Dr. Chadsey said he believed that not more than one-third of the births under the care of physicians were reported to the Bureau of Records, and that midwives never reported them. He would suggest that each physician and midwife

should keep a record of births, and it should be examined at stated times by the inspectors.

Dr. Rodgers remarked that the author of the paper had gone over the ground so thoroughly, there was not much room for discussion.

Dr. Hubbard said he approved of the suggestions of the paper, but, until the Legislature passed a new law giving the Health Board power to make a monthly house-to-house visitation, an exact record of births could not be obtained. Many women are not attended by any physician, while others employ midwives who do not report the births.

Dr. J. Foster inquired of Dr. Russell if the Board of Health had the power to make a house-to-house visitation. Dr. Russell replied that, it had, but a change in the law would be necessary to enable it to employ a sufficient number of inspectors—it would require three hundred inspectors to do the work properly. He thought the physicians should do their duty, althoug hhe favored a new law. He also stated that the German midwives reported births better than physicians.

Dr. E. Eliot said he knew physicians who never reported births. He thought that, if they were made to pay the legal fine of ten dollars, they would be more prompt in reporting.

Dr. Russell said that in some cases it was almost impossible to discover the medical attendants. Suits had been commenced against several physicians, and some had paid the fine.

Dr. Van Kleek said that it seemed hard that those physicians who habitually reported their births should be fined, if now and then they were a day or two behind the prescribed time—ten days; while others, who never make any return, escape.

Dr. Hubbard said he believed in the method adopted in London, of dividing the city into a number of districts, with an inspector for each, and stated that it worked well in Providence, R. I.

Dr. Roberts (Vice-President) said that the system inaugurated by the late able Sanitary Superintendent was pursued,

he was confident, with equal zeal, fidelity, and success, by his successor, the present Registrar of Vital Statistics, to whom we had listened with so much pleasure. He did not quite share in the estimation in which statistics were held by many, compared with the practical duties of a health department, and thought that the registration of marriages, the birth and still-born registration, might be advantageously referred to some other department, as adding considerably to the expenses of clerk-hire and publication, which, cramped as the Health Department was for means, might be better spared, as not essential to the interests of true sanitary science, and objects rather of curious inquiry than practically important to the conservation of the public health. The record and tabulation of diseases and deaths annually is an important feature in all boards of health, and cannot be dispensed with.

He said: Permit me to point out what I consider the essential and imperative duties of a health department: First, to prevent, as far as possible, the introduction of disease into the city from without. Some diseases, as cholera and yellow fever, are always imported; others are often imported, as typhus and relapsing fever, and small-pox, although they may arise here from domiciliated germs. Now, to meet the first of these requirements, so far as contagion from abroad is concerned, a rigid enforcement of quarantine can alone be depended on, and with this, so far as contagion from abroad is concerned, the Health Department has but little to do.

But, when it is ascertained that a case of disease has made its way into the city from anywhere, the best efforts of the Board should be, and are, directed to preventing any further spread. Statistics showing how far and how successfully this has been done would be undoubtedly valuable, as proofs of success and guides and incentives to further effort. That this is possible and essential, is shown by the success of the efforts made to prevent the spread of the cattle-disease in 1868, and the consequent avoidance of sources of disease in the use of unwholesome food; a point in which, in other ways, the Health Department exerts a very wide-spread and salutary influence.

Second, in every large city in which zymotic diseases have long prevailed annually as endemics, and have become domiciliated, there remain dormant, at all times, seeds of contagion which at intervals awaken to activity and exert their deleterious influences. Now, where these seeds lurk, or what they consist of, may not be known; but, if, as is strongly suspected, they either exist in decomposing filth and garbage, or find a nidus there for their ova, it becomes the paramount duty of a Health Department to destroy, by all known means and methods of disinfection, these seeds of infection and contagion.

Third, and most important consideration of all. Inasmuch as contagious diseases arise, not only from the zymotic extension and diffusion of germs, but that these germs escape and are diffused within limited spaces of the surrounding atmosphere, from the bodies of the infected themselves, their skins, breaths, and, above all, their excreta, which is particularly the case in diseases which have their seat in the mucous membranes of the intestines (as typhoid fever and cholera) and adhere to clothing, etc., it becomes the duty of the agents of the Health Department to cause the freest ventilation and purity to be maintained in the houses of the sick—tenement-houses especially—to isolate some in upper chambers, with the least possible contact with the well, and to remove others to hospitals.

Besides the duty of disinfecting above specified, these decomposing matters should be promptly removed, which is not done under the existing municipal arrangements. The street-cleaning should be under the control and inspection of the Health Department, which, unfortunately, it is not; the latter having little or no share in effecting it. Until the two departments are conjoined under a common head, and thoroughly and simultaneously performed, sources of disease will continue to be rife in our midst.

The Board of Health, I see by the papers, have petitioned for the means of carrying on their work: reciting their labors, sanitary, not statistical; and, in the *interim*, they stand comparatively still, and their employés are unpaid. If you would

have fewer items for the statistician, you must continue uninterruptedly the practical work of the department.

You have, my fellow-academicians and fellow-citizens, a Board of Health, under the quiet but sagacious vigilance of a head whose sole objects are the sustaining of the honor and credit of the Board, and the safety of the city, whose competent assistants are on the watch daily and by night, when you are sleeping, unsuspicious of danger, to keep the wolf from the door, and secure your health and that of your families. This, from my own knowledge and experience, I assert is done to the very best of its means and ability. Look to it that these means are never crippled, and this ability never impaired. Beautify all your parks and squares, etc., if you will, with millions annually drawn from the city treasury; but not at the expense of the custodian of the public health and safety.

The Board of Health should be able to say, in the proud language of a noble Scottish family, "Thou shalt want ere I want." Salaries equivalent to the social and professional positions of those employed, many of whom are refined and intelligent gentlemen, in their laborious, disgusting, and dangerous line of duty, should be punctually paid them.

The street-cleaning is now in charge of the Police Department.

The Academy then adjourned.

Stated Meeting, November 2, 1871. Dr. E. R. Peaslee, President, in the Chair.

Drs. T. M. Cheesman and J. S. Monell were elected Resident Fellows.

The paper for the evening, on "Some Points in the Treatment of Fractures of the Shaft of the Os Femoris," was then read by Dr. F. P. Foster.

The Academy then adjourned.

STATED MEETING, NOVEMBER 16, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

THE President announced the reception of the *Edinburgh Medical Journal* for November, 1871, also several Norwegian books presented by the Royal University of Norway, at Christiania, as contributions to the library.

Dr. J. C. NOTT then read the paper for the evening, on "The Natural History of Yellow Fever."

The Academy then adjourned.

STATED MEETING, DECEMBER 7, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DR. CHARLES W. PACKARD was elected a Resident Fellow.

Dr. BELL introduced Dr. L. D. PILCHER, Surgeon U. S. Navy.

The President announced the reception of "The Proceedings of the Royal Medical and Chirurgical Society" of London, Vol. VI., No. 7, also the General Index for Vols. I. to LIII. of the Transactions.

The Secretary read a letter from the Trustees of the American Museum of Natural History, Central Park, inviting the Fellows of the Academy to visit the museum on Mondays and Tuesdays, when it is closed to the public.

"The Natural History of Yellow Fever" was then discussed by Drs. J. C. PETERS, A. N. BELL, NOTT, PILCHER, G. M. SMITH, FLINT, and WALSER.

The Academy then adjourned.

STATED MEETING, DECEMBER 21, 1871. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

THE President announced the reception of the following contributions to the library, viz., "La Lithotritie et la Taille, Guide Pratique pour le Traitement de la Pierre," and "Col-

lection de Calcules Urinaires et d'Instruments de Chirurgie," by J. Civiale, M. D., of Paris, presented by his son; also a pamphlet, on "Deep Sea Dredging," by Prof. Agassiz.

The Secretary read a letter from Dr. Prosoroff, of St. Petersburg, Russia, describing an instrument for the transfusion of blood and for other purposes. A drawing of his instrument was presented and examined by the Fellows present.

The discussion on "The Natural History of Yellow Fever" was continued.

The Academy then adjourned.

STATED MEETING, JANUARY 4, 1872. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

Dr. J. F. CHAUVEAU was elected a Resident Fellow.

The President announced the death Dr. Henry D. Bulkley, an ex-President of the Academy, January 4, 1872, aged sixty-eight years.

The President announced the reception of a work entitled "Traité des Operations des Vois Urinaires," par le Docteur Reliquet, of Paris.

The annual election then took place, at the close of which the President announced that it had resulted in the choice of the following officers:

Vice-President, Dr. SAMUEL S. PURPLE.

Trustee, Dr. I. E. TAYLOR.

Committee on Admission, Drs. G. M. SMITH and E. H. JANES.

Committee on Medical Ethics, Dr. J. C. PETERS.

Committee on Medical Education, Dr. F. A. BURRALL.

Dr. J. R. LEAMING then read the paper for the evening.

RESPIRATORY MURMURS.

By JAMES R. LEAMING, M. D.,
ONE OF THE VISITING PHYSICIANS TO ST. LUKE'S HOSPITAL, NEW YORK.

Mr. President and Fellows of the Academy:

Laennec recognized both bronchial and pulmonary breath-sounds, and explained them as being caused by air-friction. In describing pulmonary respiration, he says: "On applying the cylinder, with its funnel-shaped cavity open, to the breast of a healthy person, we hear, during inspiration and expiration, a slight but extremely distinct murmur, answering to the entrance of the air into and expulsion from the air-cells of the lungs. This murmur may be compared to that produced by a pair of bellows whose valve makes no noise, or, still better, to that emitted by a person in a deep and placid sleep, who takes now and then a profound inspiration" (Forbes's Laennec, p. 29); and the translator adds in a foot-note: "It will be most easily and distinctively perceived by applying the naked ear to the chest of a child." Laennec's view is theoretical. Indeed, at the time he wrote, the minute anatomy of the lung, and the constitution of the residual air, were not known. Subsequent opinions have been influenced more or less by Laennec's, especially in this, that all respiratory murmurs are considered to be air- and tube-friction sounds. Many differ from him as to the seat, but nearly all agree with him as to the mechanism. M. Beau, of Paris, placed its seat in the pharynx; Dr. Sanderson, of Edinburgh, in the rima glottidis. Skoda, of Vienna, considered vesicular murmur as occurring only in inspiration, and being caused by air-friction, and he likened it to the noise one makes in forcing the air through the nearly-closed lips. He denies that the respiratory murmur has any thing to do with the vesicular breathing, which, he says, is a purely bronchial sound. Andral called it a sound of *pulmonary expansion* or *vesicular respiration*, designating its seat, and giving it a name, which it still holds. Many speak of vesicular and respiratory murmurs as interchangeable terms, as the late Dr. Hyde Salter, who placed the seat

of the respiratory or vesicular murmur in the convective system, and mostly in the sub-pleural, minute bronchioles (*British and Foreign Medico-Chirurgical Review*, July, 1861). Dr. Waters, whose prize essay "On the Minute Anatomy of the Human Lung" has done so much to increase our knowledge on this subject, describes the mode of connection of the bronchioli with the air-sacs. The opening sometimes is, as it were, a hole punched out, clean and round, and the air, passing in and out, must make a sound much in the same way as is done in a toy tin-whistle. The late Dr. Cammann, of this city, believed the cause of the murmur to be the passage of air into the air-sacs and out again. Dr. Williams, after speaking of portions of the chest where blowing-sounds are heard, goes on to say: "Then there is the vesicular respiration, which is heard in most other parts of the chest; it is a diffused murmur caused by the air penetrating through the minutest tubes, and into their numerous vesicles or cells." Dr. Gerhard, of Philadelphia ("Lectures on the Diagnosis, Pathology, and Treatment of Diseases of the Chest"), says: "The sound of air entering the vesicles is different from that caused by its passage through the tubes, and the former is, therefore, known as the vesicular sound, the latter as the tubal or blowing sound. The vesicular sound is often called a murmur, from its softness and diffusion over a large space, and cannot be produced unless the vesicles are healthy or nearly so." And, again, he says the cause of difference "seems to be the different manner in which the air impinges upon the vesicles and tubes. But the vesicular sound is in part owing to the vibration of the air, and in part to the noise produced by the dilating of the vesicles themselves."*

Dr. Walshe represents the natural respiratory murmurs as caused by inspiration and expiration, for which there is usually a healthy type, "commonly termed—*a*, pulmonary or vesicular; *b*, bronchial; *c*, tracheal; *d*, laryngeal; *e*, pharyngeal,

* Dr. Gerhard's views of the mechanism of respiratory murmurs are very similar to those put forth in this article.

according to the part of the respiratory apparatus from which the sounds audible externally are transmitted." Dr. Corrigan divides the sounds heard in auscultation into "simple sounds or murmurs, and compound sounds or rattles. . . . All the sounds heard in the chest belong to one or the other of those two kinds; and, if, when you hear a sound, the exact nature of which you may be in doubt, you will first refer it to its class, your labor in determining what it is will be very much diminished." The American editor of "Stokes on the Chest" describes vesicular murmur as that "of a soft and gentle, or, as it has been otherwise described, a mellow, continuous, gradually-developed, breezy murmur, unattended with a sensation either of dryness or humidity; and we are properly cautioned by M. Fournet and his reviewer not to expect a character of sound which conveys the notion of a successive dilatation of separate vesicles, or, as it is sometimes called, pure and vesicular." Dr. Hyde Salter says: "There is another reason, to which I have not referred, which makes me think that the respiratory murmur must have a tubular or *quasi* tubular seat, and cannot be formed in the air-cells; it is, that fine crepitation, such as that of pneumonia, *supplants* it; it does not merely drown it, it supplants it, the two do not coexist;" and farther on: "If, then, pneumonic crepitation is a veritable tube-sound, and its seat the microscopical tubes immediately subtending the air-cells, the supplanting and destruction of the respiratory murmur by it would show that this latter has an identical seat, and is therefore a tube-sound." As Dr. Salter is one of the latest, as well as one of the most brilliant writers on this subject, perhaps he represents the advanced views of the profession. Some consider the respiratory murmur as having a single seat and cause, while others recognize its composite character. Others speak of bronchial breathing, of the vesicular character, of the pulmonary quality of the respiration, but attempt no analysis. The sounds formed within the broncho-respiratory are frequently confounded with those formed within the true respiratory system.

A clear understanding of this whole matter will make it necessary, as preliminary, to consider, first, differences in the minute anatomy of the *tissue* of the lungs, and of the bronchial system; secondly: in the circulation of the lungs and of the bronchial system; and, thirdly, the characteristics and constitution of the residual air, its object and office. The bronchial system may be called the broncho-respiratory system, and the pulmonary, the true respiratory system. They differ in almost every respect. The office of the broncho-respiratory is to convey air; the true respiratory system is where the great function of vitalizing the blood is perfected. The bronchial system is characterized by cartilage in its fibrous sheath; in the upper part, in nearly perfect rings, but, as the tubes pass into the lung-structure, the cartilage gradually loses this form, and appears merely as deposits occurring at irregular intervals, down so far as to where the residual air constantly distends the tubes with force. The mucous membrane, also, of the broncho-respiratory system, is different from that of the true respiratory, in this, that it is ciliated epithelial mucous membrane, while the other is of tesselated basement epithelium.* The circulation also is entirely different. The convective system is supplied by the bronchial arteries; the pulmonary substance by the pulmonary artery. The nutritive arteries arise from the bronchial arteries, but have no accompanying veins. Their blood, after performing the proper office of nutrition to the pulmonary tissue, is at once reaërated, and passes into the venous radicles of the pulmonary vein prepared for systemic circulation.† The bronchial veins return all the blood of the bronchial arteries; the nutritive arteries have no veins. Their blood is reaërated where they do their work, and it finds its way into the venous radicles of the pulmonary vein as arterial blood. This anomaly in the circulation is of great interest in explaining physio-

* The ciliated columnar epithelium, so characteristic of the bronchial mucous membrane, ceases at the commencement of the alveoli. (Dr. Waters "On the Chest," 1868.)

† Dr. Waters "On the Chest," pp. 16, 17. Also, Stricker's "Histology," 1872, p. 443; Niemeyer, vol. i., p. 60; Wilson's "Anatomy," p. 514, 1859; Gray's "Anatomy," p. 720.

logical causes and pathological effects. In pneumonia, it is the nutrient artery, accompanied by its ganglia of the organic nerve, lymphatics, etc., that preserves the life of the part, and governs the whole process of resolution. We can all remember the anxiety of practitioners, in the past, to prevent abscess and gangrene of the lung after inflammation. But time, and a more careful study of the natural history of the disease, have proved to us that gangrene and abscess are rare accidents, even when no treatment is had. This peculiar arrangement of the nutrient artery gives us an early knowledge, in many cases, of commencing phthisis. Occupation of the air-sacs by tubercle interferes with the circulation, and blood is thrown back upon the bronchial artery, and the result is bronchorrhagia, a conservative act; like the application of leeches, it sets the absorbents actively at work to remove the cause. And, in this way, cases of early phthisis are self-cured, or, at all events, ameliorated, and the physician is guided in his treatment.

This singular fact in the circulation was discovered by the late Dr. Cammann, in making his experiments to prove the non-anastomosis of the arteries of the lung. Using a colored fluid suitable for fine injections, he found that, when he injected the pulmonary artery, the fluid returned easily by the pulmonary vein; but, injecting the pulmonary vein, the fluid not only passed into the pulmonary artery, but, if the injection was carefully continued, it would also find its way into the bronchial arteries. Then, again, injecting the bronchial arteries, he found that the fluid after a little time passed into the pulmonary vein; this proved that there was communication between the bronchial arteries and the pulmonary vein, but not with the pulmonary arteries.* This was shortly after 1840, and before, I believe, any experiments had been made in Europe, in regard to this circulation. Since then several observers have come to nearly the same conclusion. Drs. Williams and Adriani believe "the vessels of the bronchial mucous membrane terminate in the pulmonary veins, and those of the deeper plexus in the bronchial veins." Dr. Waters says,†

* Communicated to me by Dr. Cammann.

† "Minute Anatomy of the Human Lung."

after explaining his experiments, which were very full and minute: "That a distinct and free communication exists between the bronchial vessels and the pulmonary veins, admits of ocular proof. I have seen, with the aid of the dissecting microscope, the small vessels passing from the outer surface of the bronchial tubes, and forming a small trunk, which terminated in a pulmonary vein;" and again, "It may be said that such a view militates against the generally received opinion of the purity of the blood returned to the left side of the heart, for, if the bronchial blood is poured into the pulmonary veins, it is returned to the left auricle without undergoing the process of aëration. I would answer that the view I have taken is supported by anatomical facts, a basis on which all physiological theories should be founded." I remember that Dr. Cammann, also, could not reconcile the incongruity of the apparent fact that venous blood passed directly into the aërated blood of the pulmonary vein, and then to the left heart. Both of these gentlemen overlook the fact that the blood from the nutrient artery *passes through capillaries* in the true respiratory system on its way to the radicles of the pulmonary vein, and, of course, is reaërated. Dr. Robert Lee, if my memory serves me (for I have not the paper at hand), says that the extension of the bronchial artery, after it has quit company with the vein, receives additions from the mammary and intercostal arteries, and has the proper title of nutrient artery.

Am I not warranted in holding that there is a complete difference in the blood-vessels of the convective and of the pulmonary systems? The nutrient arteries of the bronchial system have their venæ comites; the nutrient arteries of the true respiratory system have no accompanying veins, but pass their blood reaërated directly into the pulmonary vein, prepared for systemic circulation. The nutrient artery, having its origin mostly from the bronchial, is no exception to the rule of complete difference in the two systems, for in its office it belongs wholly to the true respiratory. The vessels of the bronchial system are the bronchial arteries and veins; the

vessels of the true respiratory are the pulmonary artery and vein, and the nutrient artery of the lungs.

Where the bronchial system ends the pulmonary begins—it is where cartilage ceases and alveoli commence. The structure of the true respiratory system is composed of terminal bronchii and of the air-sacs, that is, wherever alveoli are found. Its object is, aëration of the blood. It is greatly distensible, and in this differs from the convective system, which is but little so. The bronchioles have alveoli developed in their sides, but not to the same extent as in the air-sacs, which are but a skeleton net-work for the convenient spreading out of alveoli, with their rete mirabile of capillaries. The terminal bronchus enlarges at its end, and the air-sacs are developed from this enlargement, according to Dr. Waters, as a cluster of leaves are sometimes from the end of a twig. From six to thirteen of these air-sacs are in connection with the enlarged end of a terminal bronchus, and this little cluster forms a lobulette—a complete type of the whole lung. Each lobulette has its terminal bronchus and air-sacs for the development of alveoli, its twig of pulmonary artery and vein, its branch of nutrient artery, with the accompanying gangliæ of organic nerve, lacteals, absorbents, etc. A collection of lobulettes form a lobule, and a number of these constitute a lobe. The fibrous bands of the bronchial sheath, both of the white and yellow varieties, are continued, though with great tenuity, through the terminal bronchi into the air-sacs. They surround and give firmness to the frame of each alveolus. Niemeyer affirms that muscular fibres are present in the true respiratory system. In emphysema the air-sacs lose their power of contraction, and become dilated, causing great suffering and disability to the patient. Time and freedom from catarrh allow the function of contraction to return. Can this occur in any other tissue than the muscular?

Physiologists describe residual air as filling the respiratory system as high up as the third or fourth divisions of the bronchiæ. It not only fills the true respiratory system, but distends it. The elements of the distending force are: atmos-

pheric pressure, muscular contraction, rarefaction, and the laws of diffusion of gases, and that of affinitive attraction between oxygen and venous blood. The residual air occupies its position with such persistence so as to be with difficulty dislodged, even after death. It keeps its place with vastly greater tenacity, during life, when each element of force is in active operation.

During inspiration, the contraction of the diaphragm increases the capacity of the chest, and at the same time the epiglottis is raised, and the weight of the atmosphere operates actively in dilating the lungs. Rarefaction of the newly-inspired air takes place upon its immediate and intimate admixture with the residual air, and is the third element of dilating force. The residual air is estimated to be 170 cubic inches, and the inspired air at 20. At each inspiration, therefore, the residual air will be increased about one-tenth part in dilating power, *plus* the rarefaction of the inspired air. But the peculiar elements of this expanding force are, the laws of the diffusion of gases, and that of the affinitive attraction between the unaërated blood-globules, in the capillaries of the rete mirabile of the alveoli, and the oxygen, which is equally distributed throughout the residual air. Chemistry demonstrates that gases differently constituted in certain relations instantly intermix when brought together. The inspired air and the residual air present these differences. Air entering the convective system moves in a body through the bronchial tubes, till it meets the residual air, when, the law of the diffusion of gases operating, immediate admixture takes place. The residual air is instantly renewed with oxygen, in accordance with this law. The inspired atmospheric air moves through the convective system, as far as the fourth division of the bronchiæ, with no other resistance than the friction of the tubes. When it meets the residual air, it is immediately consumed, as it were, and does not accumulate, causing resistance. On this account the inspired air moves with increasing velocity, producing air- and tube-friction murmur. Tidal air in health is only heard in inspiration. Velocity of the moving air in the

tube is the cause of murmur. Any one may demonstrate this fact by breathing through a tube gently, when there will be no murmur, but, if he increase the velocity of the moving air, he will get sound, which will be increased in sonority and raised in pitch just in accordance with the rate of motion. In health, in unconscious breathing, expiration is not heard, and we know by experience that, when it is heard in unconscious breathing, there is disease—phthisis or emphysema. A murmur may be produced at will, by hurrying the respiration. It is heard in systemic diseases, like cholera, or in diseases of particular organs, as in cardiac apnœa, or Bright's small kidney. The cause of murmur, in air moving in a tube, is *velocity*, increasing the air- and tube-friction.

Prof. John W. Draper has given a convincing explanation, based on accurate experimentation of affinitive attraction in the systemic capillaries, as one of the efficient causes of the circulation. The same power operates in the pulmonic circulation, but with this important addition, that the attraction is not alone in the pulmonic tissues and the blood, but principally in the venous blood and the oxygen of the residual air. This is the cause that brings the venous blood and oxygen together, in order that the blood may be purified and fitted to continue the life of the body. Let us endeavor to comprehend the intricate mechanism of the respiratory act. Inspiration has taken place—twenty cubic inches have been added to the residual air, evenly and equally admixed—dilatation has taken place with force, and is continued and increased by the rarefaction of heat. The true respiratory system, by its muscular power, contracts, antagonizing the dilating residual air, and compressing it with force. Each particle of pure air, acknowledging its attraction for the venous blood, presses up to the alveolus, through the struggling mass, and rushes to the blood-globule in the capillary—makes the interchange—gives up its oxygen, and receives in return detritus and carbon materials, loses its attraction, becomes passive, but is crowded back by other eager particles pressing forward, until finally it finds itself well up in the bronchus, with its filthy load,

whence it is expired. The blood-globule from the pulmonary artery, entering the capillary of the alveolus, hurries along through the rete mirabile, drawn by its affinity for oxygen, till it meets a particle of pure air, makes the interchange, loses its activity, but is pushed onward by other globules pressing forward from behind, till it finds itself in the venous radicle of the pulmonary vein, fitted for systemic circulation. The movement of the blood-globules is much assisted by the contraction and relaxation of the muscular fibres of the true respiratory system in respiration. Different bundles of these fibres, in the healthy lung, contracting and relaxing in succession, give not only a living vibratory motion, which assist in hurrying the globules along, but produce a susurrus, which, being heard at the chest-wall in multitudinous concert, is true respiratory murmur. If these are facts in minute anatomy and physiology (and they hardly admit of dispute), they prove that the residual air, as a body, has no more currents than has the bottom of the deep sea. No change can occur except molecular, and none other is necessary. The law of diffusion of gases assures the comparative purity of the residual air, as well as its constant and guarded *impurity*, which is so necessary for the accomplishment of the vital act. The circulation would not go on, if each blood-globule should immediately come in contact with pure air, for then it would lose its impelling force, and, all of the blood-globules alike losing their attraction, there would be stasis. Instead of this, both in the blood and the residual air each globule and each air-particle moves in perfect order, never in each other's way. This shows how the individual may live in bad air for a time, resisting its evil tendencies, and even that of poisonous gases. It shows also why medical inhalations fail in their object. Medicated vapors have little or no admission into the residual air. Even oxygen gas, which is sometimes serviceable, can only supply atmospheric deficiencies. It can neither do the harm nor the good that has been predicated for it. An animal may even live for a time in pure oxygen gas, the active interchange taking place between the gas and the blood restoring the necessary grade of impurity in the residual air.

If, then, the only change or motion that is possible in the residual air be molecular, what becomes of the theories of air- and tube-friction murmurs, whether in the smaller bronchiæ or the air-sacs and alveoli, as cause of the so-called vesicular murmur? Are they not physical impossibilities? And, too, what becomes of the theories of the mechanism of crepitant *râle?* If there is no motion but the molecular, there can be no bursting of bubbles in the microscopic tubes, and that theory falls. If the residual air constantly and forcibly distends the true respiratory system, how can the bronchioles and air-sacs come together, to be separated by each inspiration of fresh air, so as to produce fine crepitant *râle*? This theory, likewise, supposes a physical impossibility. All theories, whether of vesicular murmur or crepitant *râle*, which ignore the presence of the residual air, are of necessity incompetent. The fact that residual air has none but molecular motion may be demonstrated by a distensible bag, as of India-rubber. While it is being forcibly filled with air, there will be air- and tube-friction murmur at the mouth only, where the air moves in a body with velocity. The body of air in the bag will be increased by particles of air sliding in among each other, and over those sticking to the wall of the bag; there is no friction against the wall, and consequently no murmur. But there will be resisting vibratory sound in the walls of the tense dilating bag; different, however, from that of the contracting true respiratory murmur in this, that it is only heard during dilatation, while the other is continuous, because owing to active muscular contraction. Dr. Hyde Salter says, after speaking of the occupancy of the true respiratory system by residual air, and that about twenty cubic inches of atmospheric air are added at each inspiration: "Each air-cell is, therefore, a tenth larger at inspiration than at expiration. Now, it is inconceivable that this slight variation in the capacity of these shallow, open concavities should be attended with any sound. I cannot conceive it possible. For, be it remembered, that the air-cells are not nearly-closed cavities communicating by constricted orifices with the general cavity of the lobular passage,

but wide-mouthed and patulous like a teacup. And, be it remembered, too, that in respiration the air is not pumped out of and into the cells, but, as they undergo this slight change of volume, a small part of their contents passes just without them; and then again, on their recovering their capacity, from without just within them, if one can speak of 'within' and 'without,' in reference to such slight interchange of situation. For, really, the renovation of the air in the tissues of the lung does not depend on its actual removal, but upon the law of the diffusion of gases."

This reasoning is cogent. It proves that there is no motion in the air-sacs and alveoli to produce air- and tube-friction sound, and yet he attempts to show that there is such motion in the smaller bronchiæ and intralobular passages. He says: "But while the movement of the air at each alveolus would be so slight, so almost inappreciable, the collective expansion of all the alveoli common to a lobular passage, and the consequent abstraction of air from the general cavity, would be considerable, and would create a considerable rush of air into the lobular passage to supply its place; for the modicum of air, however small, appropriated by each dilating air-cell, would of course be multiplied by the number of cells communicating with the common axial cavity of the lobular passage."

Dr. Salter's able reasoning shows that there is not enough motion in the alveoli or air-sacs to cause sound, and the same reasoning applies with equal force to the air in the bronchioles and intralobular passages. The residual air occupies these passages just as well as it does the air-sacs, and is compressed in one as well as the other; one-tenth is added at each inspiration to the whole body of residual air, and Dr. Salter himself has said that these small bronchial tubes were largely distensible; consequently the velocity of motion in these passages where alveoli are developed must be too little, if there be any at all, to produce any sound. Dr. Salter's argument to prove that the seat of crepitant *râle* and the seat of respiratory murmur are the same—"the *râle* supplants the true respiratory murmur, the two do not coexist"—heretofore quoted,

is convincing. Had he placed the *seat* in the air-sacs and alveoli as well as in the terminal bronchioles, he would have been correct, for then he must have acknowledged that it *could not be by tube- and air-friction*, and he would have been forced to accept the true explanation, that of dilatation and contraction. Crepitant *râle* indicates the commencement of the process of inflammation, and it supplants the true respiratory murmur. Let us study the evidence in the light of the true respiratory murmur.

If one have lately examined the chest of a person in health, and have noted the murmur in its fullness and perfection, and should be called to see him suffering from a chill, with pain in the head, back, limbs, etc., and should again examine the respiration carefully, he will probably still hear the true respiratory murmur, but it will be obscured or muffled. All the capillaries of the lung are crowded with blood, which is the explanation of the muffled murmur. If he wait a few hours, he will find the true respiratory murmur absent, and, in place of it, the fine crepitant *râle.* The congestion of the capillaries of the lung still remains; there is scarcely a perceptible difference in the percussion-note; the residual air still occupies its seat in the true respiratory system, and it still continues to dilate the air-sacs, alveoli, and terminal tubes. Whatever change has taken place, must have been at the seat of the true respiratory murmur.

In tissues that may be seen, what is the first result of inflammation? Is it not that plastic material is thrown out into the connective tissue? This, also, must take place in the lungs. The connective tissue of the lungs, delicate as it is, has been filled with plastic material. It has become thickened and stiffened, it cannot contract, and the true respiratory murmur is gone, but it must yield, though unwillingly, to the dilating force of the residual air, separating newly-formed plastic exudations, causing sound—fine crackling—which is crepitant *râle.* If he wait a few hours more, and examine again, he will find that crepitant *râle* as well as true respiratory murmur has gone, and all is silent, or there may be

bronchial or tubular breathing. Exudation has been poured into the true respiratory system, and consolidation is the result. The seat of crepitant *râle* is now become the seat of exudation. If I have studied this matter as correctly as I have carefully, this is the process gone through with, and is the true mechanism of crepitant *râle*.

In this paper I have endeavored to show that the bronchial respiratory system is entirely different from the true respiratory system in anatomy, physiology, object and use, and that the physical signs of pathological change are equally distinct and different.

The composite character of the respiratory murmur must be made evident, analytically as well as synthetically. The two elements, different in cause, character, and seat, must be individually studied in order that we may correctly understand their significance in pathological changes. We may present their union and the result to the eye, thus:

Broncho-respiratory murmur.	Respiratory murmur.
True respiratory murmur.	

The reasons for introducing a new terminology are, that broncho-respiratory and true respiratory are descriptive, and indicate the seat of the murmurs. The term vesicular murmur was applied by Andral, supposing that it described the minute anatomy of the seat of the murmur; but later investigations show that the term is misapplied, for there are no structures that may properly be called vesicles in the lungs. Again, the terms vesicular and respiratory have been applied indiscriminately, and their present use would lead to confusion and misapprehension.

In order to practically study these murmurs, it will best be done by selecting a healthy person about twenty-five years of age, with perfectly developed chest, and with muscles not hardened by manual labor.

RESPIRATORY MURMURS.

Placing the ear lightly yet firmly upon the chest, allowing the head to rise and fall with the respiration, listen to the breath-sounds. The tidal-air murmur will first catch the ear as modified by the true respiratory murmur, and, as has been described, is like the sighing of the trees over our heads in the forest, when the boughs are gently stirred by the breeze. The character and quality of the respiratory murmur depend upon the absence or excess of one or the other of the composing elements. If the true respiratory murmur be maximum in fullness, the tidal-air sound will be short, only heard in inspiration, and will be of the soft, breezy character described as gently sighing.

While, if the broncho-respiratory be in excess, the tidal-air sound will be harsh, raised in pitch, and will be heard both in inspiration and expiration, and becomes a sign of disease as the other is of health.

BRONCHO-RESPIRATORY MURMUR.

Broncho-respiratory murmur may be studied by forcing the breathing, when it will be heard in both inspiration and expiration, and its harshness, loudness, and pitch, will depend upon the force given to the respiration. This murmur may be heard in its perfection, in the chest of a child before the true respiratory murmur has been developed.

TRUE RESPIRATORY MURMUR.

The ear accustomed to auscultation, after a few moments of concentration of the attention upon the respiratory murmur, will recognize its dual composition. If the chest be perfect in condition, the tidal-air sound will be heard in inspiration only —soft and short, like breathing gently through the closed teeth—while the true respiratory murmur will be continuous, increasing in fullness in inspiration and diminishing in expiration. It is of low pitch, and is like the roaring of the sea at a distance, the waves breaking on an even shore of sand; or,

better still, like the sound made by bees in cold weather, when the hive is tapped with the finger. It is like the innumerable vibrations of the wings of bees, increasing to maximum in inspiration like the breaking waves on the near shore, and decreasing in expiration like the receding waves. If the breath be held, this murmur may be heard without admixture, and studied separately. I believe the sound is the susurrus of the delicate muscular fibres of the true respiratory system, contracting and relaxing over the dilating and resisting residual air. If the breath be held after a full *inspiration*, the murmur will be at its maximum; if it be held after *expiration*, it will be at its minimum fullness. It cannot be exaggerated, as has been said of the so-called vesicular murmur. If the true respiratory system be unduly dilated, it loses its power to contract on the residual air, and the murmur wholly ceases. This is a sign of emphysema, and is proof of the muscular cause or origin of the sound which may return again after rest. Dr. Walsh considers no theory of the cause of respiratory murmurs to be well established. But he says: "The muscular actions going on in the chest will give rise, in some persons, to a peculiar buzzing, rumbling sound . . . . it is continuous, not rhythmical with respiration; and rather increases than diminishes in intensity when the breath is held."

This murmur only commences to be developed in the child at eight years of age, becomes recognizable at twelve, but is only fully developed at maturity. A beginner in auscultation may recognize true respiratory murmur in a good subject with ease. But, when the chest has lost its excellent quality as an acoustic chamber by physical changes, resultant from inflammation, or other causes, the expert ear alone can arrive at diagnostic truth.

These facts, instead of being a matter of discouragement, should induce beginners to pursue auscultation with untiring assiduity, knowing that the end will crown them as masters in physical diagnosis. The ability to recognize true respiratory murmur under any conditions, to analyze its quality, and measure its power, gives its possessor the means of knowing

even the approach of that insidious disease, phthisis, and suggests the method of prevention. The true respiratory system, air-sacs, alveoli, nutrient artery, ganglia of the organic nervous system, with absorbents, etc., all require active use for the prevention of disease. Phthisis does not begin in the *lower* part of the lungs, which are constantly and actively in motion. If we insure the same kind of exercise in the *upper* part, we prevent and may even arrest incipient disease.

For more than ten years respiratory murmurs have occupied my diligent attention. The views put forth in this paper were not hastily formed. Some of them are new, and may receive the harsh judgment innovations ever provoke. I do not deprecate criticism, but I am not insensible to the opinions of my professional brethren. I earnestly desire their approval. Many, on whose judgment I rely, believe in true respiratory murmur as well as myself. But, wishing, above all things, for the establishment of truth, I submit the whole subject to this learned Academy, and through it to the profession.

Stated Meeting, February 1, 1872. Dr. E. R. Peaslee, President, in the Chair.

The President announced the reception of a copy of the Report of the Board of Health of New York City for 1871.

Dr. Theodore Walser read a paper on "Quarantine Regulations," of which the following is an abtract.

ON QUARANTINE REGULATIONS OF THE PORT OF NEW YORK.

By THEODORE WALSER, M. D.

Mr. President and Fellows of the Academy.

All prohibitory regulations must necessarily be based upon the etiology and *modus propagandi* of each disease that comes under the provision of quarantine laws.

You will be pleased, therefore to bear with me if I presume to review the history of yellow fever, cholera, smallpox, and typhus separately, and consider quarantine regulations as applied to each.

Yellow Fever.

Character and Nature.—a. As long as we are unable tangibly or visibly to demonstrate the bioplasma or disease-germ of yellow fever, or actually know what constitutes the morbific matter of the disease, we must content ourselves, in the conception and true appreciation of its nature, with inference; by analogy this will unerringly lead us to the belief that the yellow-fever virus is of cryptogamic origin—a theory which assumes a practical importance when we consider the chemical action of fungi of all kinds upon the atmosphere; their deleterious influence upon animal life; the condition of their generation and growth—heat, moisture and darkness—and the corresponding means of their extermination—air, ventilation, light, and cold.

b. Whether cryptogamic or animalcular in its nature—yellow-fever virus can only be propagated at a continued temperature of 85 to 105.°

It cannot, therefore, exist in our latitude except during the summer months; becomes exterminated with the advent of frost; and is necessarily an exotic in its origin, an indigenous only in the tropics.

The domestic origin of yellow-fever virus, and its spontaneous reproduction under certain atmospheric and telluric conditions in our own climate, has been the favorite theme of our older physicians, either to excuse official neglect or pander to personal interest.

The exemption of every port or seaboard town, under any hygenic condition, during the war, when intercourse with infected localities was cut off, proves, however, that, if yellow fever is not carried thither, no local or hygienic condition can reproduce the specific yellow-fever virus, when once destroyed by the frost.

If not indigenous, yellow fever is necessarily a portable disease, and its portation either—

A. By Persons laboring under the Disease, and reproducing the Morbific Germ.

B. By Things—Fomites, Vessels, Merchandise, Personal Effects—transmitting the Morbific Germ in its Maturity.

A. By Persons laboring under the Disease, and reproducing the Morbific Germ.—The difficulty of separating effect from cause, and our inclination to draw our inferences from the visible and tangible, have necessarily led the superficial inquirer to look to the yellow-fever patient as the visible means of portation and communication, and thus to overlook the most important facts connected with the transmission and development of the disease.

A clear interpretation of the experience of a century, and much negative evidence, prove, however, beyond a doubt that persons and their eliminations have no agency whatever in the matter.

From the opening of the quarantine hospitals in 1798, to their destruction in 1858, over twelve hundred cases were treated, and not a single instance is on record in which the disease was communicated from the sick and their eliminations to attendants or convalescents of other diseases.

Over two hundred cases have since been treated on board the floating hospital, and yet the fever was confined to admissions from shipboard. In 1848 the victims of yellow fever on the eastern shore of Staten Island took refuge in every direction, and were taken care of in private residences and in most insalubrious localities, and yet no cases occurred except on the shore, extending not more than two hundred and fifty yards inland.

Hundreds of sick with yellow fever fled from Norfolk in 1853, from Fort Hamilton in 1858, and yet the epidemic was confined to its origin. Assuming, then, as an incontrovertible truth that persons *cannot* be the carriers of yellow-fever virus, the office devolves upon inanimate matter and infection.

B. By Things—Fomites, Vessels, Merchandise, Personal Effects—transmitting the Morbific Germ in its Maturity.—From the first appearance of yellow fever—in 1748, 1750, and 1756, in Norfolk and Charleston; in New York in 1785 to 1803, and, later, in 1822; in Philadelphia in 1792—the epidemic invariably commenced in ships, docks, wharves, and at the foot of streets in the immediate vicinity of infected vessels, and extended from this *focus* of infection only to certain well-defined limits.

These vessels had come from ports where yellow fever prevailed as an epidemic, with the disease on board while in port, and continuing in all its virulence beyond the time of incubation from the port, during the voyage and even after her arrival in the port of entry, thence extending its ravages to other vessels and the nearest shore—conclusively proving *to be infected*, and the *carrier* of the morbific germ in whatever form it may be.

To prove this assertion would only be reciting the history of every yellow-fever epidemic on this continent, first as concomitant of the slave-trade from the coast of Africa, and after the infection of the West Indies, where it became planted and indigenous—to every port north as far as Portland and south to Buenos Ayres—or repeating the etiology of every case of yellow fever in the sailor, passenger, ship-keeper, and stevedore treated in the quarantine hospitals since their establishment.

The only questions, then, to be answered, are:

a. The Source of Infection.

b. The Conditions favoring the Infection of Vessels; and

c. The Mode of Transmission of the Morbific Virus to other Vessels and the Shore.

a. The Source of Infection—the Port.—The yellow fever may prevail as an epidemic appalling and intense, or only in the occurrence of a few sporadic cases.

A vessel may become infected in either; but much more readily in the former. The port itself may be land-locked, formed by a sluggish river, surrounded by swamps and decay-

ing matter, as Havana; or it may be an open roadstead, and the probabilities of infection, therefore, correspondingly less; or else the vessel may be loaded by lighters or at the wharf, the former not excluding the latter, certainly favoring the probabilities of infection.

b. The Conditions favoring the Infection of Vessels.—The statistics of quarantine show that fully two per cent. of vessels arriving from an infected port may be considered infected; and the conditions favoring the infection of vessels may therefore be found in the length of time which vessels spend in the infected port, the proximity to the shore, the character of cargo received on board, and the condition of the vessel itself, old vessels being more liable than new ones.

Vessels impregnated with human effluvia are more easily infected than vessels carrying freight—the names of the Buzzard, Enterprise, Susquehanna, Alabama, and Tahoma, standing prominent among the list of infected vessels.

By a passenger-ship, the disease was introduced from the coast of Africa to the West Indies; a British transport from St. Thomas carried the disease in 1798 to New York; and the only infected vessels arriving here during the summer of 1862 were coolie vessels, which had discharged their living freight in Havana. The character of vessels not only favors infection, but stamps the peculiar type of the disease.

The Charles Marshall, with a cargo of putrid hides, brought the typhoid; the Susquehanna, when scurvy prevailed previously, the purpuric; the Tahoma the remittent type. In the same way the hygienic condition of the shore modifies the type of the disease.

c. The Mode of Transmission of the Morbific Virus from Vessel to Vessel, or to the Shore.

1. *By Contiguity.*
2. *By Winds.*
3. *By Fomites.*

1. *By Contiguity.*—To the law of contiguity we may attribute the prevalence of every yellow-fever epidemic in New York, commencing invariably in slips, docks, and streets, near-

est to the focus of infection, until the approach of infected vessels was prohibited by law, and in its efficient administration the introduction and prevalence of the disease prevented. What may still occur in the absence of such preventive measures we may infer from the history of St. Nazaire, in France, in 1861, when the Anne Maria arrived from Havana, having lost two of her crew by yellow fever. On board of three vessels moored close to her the crew not only sickened in the order of their proximity, but, while the vessel moored nearest to her lost four out of five, the third lost only one in seven of her crew.

2. *By Winds.*—Whatever may be the nature of yellow-fever virus, its specific gravity is obviously greater than air—as this alone can fix it to the vessel—and virulent in the degree of its density. The lower the stratum the more poisonous its influence. That by discharging and ventilating, the virus is dislodged and set free, we see in its effect upon the lightermen receiving cargo or ballast, and persons bathing in the immediate vicinity of discharging vessels. The extension of this virus so liberated, and by its specific gravity floating on the surface of the water, certainly cannot be limited, except by attenuation; and, if the formative elements are found upon the water, or, drifted by wind and wave on shore, it finds the matter or formative element on shore, it will, under circumstances resembling its tropical habitat, necessarily generate and develop. The epidemic on Staten Island in 1848, on Long Island and Staten Island in 1856 and 1858, each occurring after continued winds from the fleet of infected vessels, may be solely attributed to this mode of infection; while the epidemics on Governor's Island, occurring in 1856, 1858, and 1870, in the same locality, the point nearest the Atlantic Dock, where cargoes from vessels at quarantine were constantly received, were unquestionably due to the liberation of yellow-fever virus from these cargoes drifting on the surface, until it found on the shore of the island, or the *débris* floating near its shores, the requisite formative elements. This drifting, as snow in winter, may also account why a fence, a

row of dwellings, an artificial barrier, will arrest the progress of the disease, and why the north side of Rotten Row suffered while the south side remained exempt; on Staten Island the houses fronting the bay were infected, while others adjoining, fronting inland, remained exempt.

By Fomites.—These may be either *materials floating* NEAR, or the *cargo, dunnage, and personal effects* FROM the infected vessel.

We have unquestionably in the floating material the most prolific source of infection. This may or may not be from the infected vessel, but *débris*, decomposing porous substances constituting the formative element for the floating virus, under circumstances most closely resembling its tropical habitat.

The straw taken from the bay by the first victims on Fort Hamilton, the mattress at Gowanus, were such; though neither could be traced directly to an infected vessel.

Cargoes.—To prove the infection by cargoes is extremely difficult, because isolated cases are apt to be misunderstood, or hushed up under the apprehension of public alarm, or reproach for official negligence. In 1822 the epidemic was attributed to a quantity of sugar stored at the foot of Rector Street. Custom-house officers were reported sick of yellow fever while employed at the Atlantic Dock, Brooklyn, in 1856; and Sleight, a custom-house officer not connected with quarantine, is said to have died of yellow fever in 1848.

The law makes distinction in cargo and the character of "fomites" liable to carry disease, which is perhaps not wholly based upon facts. Rags, unwashed and from an infected port, may certainly be potent carriers; and, as the history of the J. H. Glidden teaches, a prolific source of infection, as also hides and animal remains. On the Charles Martel seven out of twelve employés sickened, who had carried the partially-decayed sheepskins on deck. But few cases are known where general cargoes and the products of the tropics proved injurious, unless in a state of decomposition.

Dunnage and Personal Effects.—That dunnage from the

hold of an infected vessel may carry the virus, we have the evidence in the Susquehanna. A watchman (?) slept in the folds of a sail removed from the hold the previous day; he sickened in five days, and died of yellow fever.

That the virus may be carried a great distance by *personal effects* we have in evidence the well-known instance of the chest of clothing carried from Martinique to New Haven, in 1794; the clothing of a dead sailor, washed in Lombardy Street, in 1822, and infecting all present; the disease of the Sinclair family in quarantine, in 1848, attributed to the clothing of stevedores sick with yellow fever; and, more recently, the case of the mate of the Crawford, who went from a vessel, which subsequently proved infected, on board of another, outward bound, the captain of which sickened three days after he slept in the bunk above the one containing the mate's clothing —in every instance the effects came from localities positively infected, without being opened and ventilated, and carried to localities favoring infection.

After careful consideration of the nature of yellow fever, conclusively proving its exotic origin, and that it cannot occur in our latitude unless carried hither by vessels and fomites, the quarantine regulations, based upon the *modus propagandi*, are of the most vital importance.

The quarantine of yellow fever can only be applied to the carriers of morbific virus—vessels, merchandise, and personal effects. The quarantine of the yellow-fever sick may be advisable or moral, but certainly not on hygienic grounds, and the only suggestion of practical utility is, the removal of the sick beyond the well-defined limits of infection, in order to secure a better result in the treatment, and a more efficient care, through the perfect immunity of its attendants.

The necessity of a quarantine of vessels from infected ports was so well appreciated in the early history of New York, that as early as 1758 a law was enacted prohibiting the approach of vessels with yellow fever on board, or coming from yellow-fever infected ports, nearer than Bedlow's Island.

The insufficiency and insecurity under existing laws demon-

strated by repeated epidemics during the latter part of the last century, and a change in medical opinions in regard to the emanation and propagation of the disease, led, in 1801, to the further removal of quarantine to the more distant shores of Staten Island, where it remained until, by the destruction of the quarantine buildings and the voice of the people, it was again removed to the Lower Bay, where now all vessels from Southern, West Indian, and African ports, arriving from April 1st to October 1st, are boarded, receiving either immediate pratique, or are quarantined.

Immediate pratique is only granted if no pretext whatever exists to detain the vessel. The quarantine is either a quarantine of observation, applying to all vessels coming from any port suspected of being infected; or implies a quarantine of from two to five days, opening of hatches, ventilation, and fumigation by chlorine, carbolic acid, or bromium—most commonly chlorine, generated by a mixture of black oxide of manganese, salt, and sulphuric acid.

If the sanitary condition of the port is more questionable, or the cargo of such a character that it belongs to the first class designated by law, the quarantine is extended ten to fifteen days; and, should the suspicion amount to positive evidence that sporadic cases occurred in the port of clearance, or any port where the vessel stopped on her homeward passage, the vessel is detained, either with her own crew, or a crew of ship-keepers, for a longer period; if all remain well, and later reports from the port of clearance show that yellow fever is not epidemic, the vessel receives a stream-permit or conditional pratique.

A vessel arriving from a port declared to be infected, by foul bills of health, or private information, is suspected of being infected, though all the crew may have been acclimated and well during the passage. After a few days' quarantine she is discharged by licensed stevedores and bonded lighters, at the Upper Bay, and no pratique granted to the vessel until sufficient time has elapsed to prove, by the continued good-health of the employés, that she is not an infected vessel.

By the discharge of vessels we have not only thorough ventilation, efficient means of disinfection of vessel and cargo, but in the continued good health of a number of unacclimated employés, exposed to the virus under the most unfavorable circumstances, a number of negative evidences, amounting to positive assurance, that the vessel is not infected.

From the same port, vessels may arrive, on board of which, cases of yellow fever may have occurred, while in port or on the passage, but no cases after the fifth day of her departure; she may be an infected vessel, but no positive evidence is adduced, since all her cases may be traced to the port of clearance; she is treated the same way as the above, receiving pratique only after every evidence manifests her sanitary condition. Another vessel arrives with new cases of yellow fever occurring six or more days after leaving port, or even after arrival; she is quarantined in the Lower Bay for twenty or thirty days after the occurrence of the last case—those discharged by lighters—her cargo only receiving conditional pratique, and the vessel only after every evidence that she is no longer an infected vessel. When, however, this evidence confirms that she is and remains an infected vessel, she is quarantined till frost, her only salvation.

It may appear strange that no provision is made for positively infected vessels, but, as frost alone exterminates the virus, and any other method would necessarily entail loss of life, quarantine till frost is the only safe method, and more readily submitted to than exactions which seem frivolous and unnecessary.

Thanks to a manifest Providence, by this course not a single case of yellow fever occurred in New York and harbor out of quarantine during the administration of Dr. Gunn, although over five hundred vessels arrived from infected ports, over two hundred and fifty cases of yellow fever were received in the hospitals, and ten vessels were declared positively infected and remained in quarantine until the middle of October.

Disinfection of Vessels.—Frost alone can positively eradicate the disease, and to apply this artificially to vessels and car-

goes is by no means impossible, but as yet only a desideratum. Fumigation by chlorine and bromine, or even the free use of carbolic acid, may aid in the disinfecting of vessels, but is by no means effectual; and the only method now employed to exterminate, and, if possible, to destroy yellow-fever virus, is free ventilation and access of light and air.

Cholera.

The literature of cholera is so voluminous, and the history of its Indian origin and prevalence, its migration over the Continent of Europe, before it reached America in 1832, so familiar to all, that I cannot presume to refer to it, except as evidence in regard to the *modus propagandi* of the disease.

By the able researches of Pettenkofer, and the experience of the pathologists of this continent, *persons* and *not* inanimate matter are proved to be carriers of the morbific virus; not, as in small-pox and typhus, by the eliminations of the skin, from person to person, but by infecting the atmosphere through the decomposing *dejecta* from the alimentary canal; not only the *dejecta* of the cholera-patient in its malignity, but even under circumstances when the cholera-sick appears to be comparatively in good health, and only suffering from a specific diarrhœa. In Europe, all quarantine seems to be useless, and neither climate nor elevation, neither the torrid nor the frigid zone, appeared as barrier to its progress. On this continent, cholera was already epidemic in Montreal and Quebec, before the first cases were admitted to the quarantine hospital from New York City (quarantine was then useless), but with the sanitary cordon of two thousand miles in width, and an isolation of ten or more days, quarantine has and may be made effective in America.

1. *By Quarantine of the Cholera-sick.*—Not, as before the destruction of the quarantine buildings, in crowded hospitals, when, as experience teaches, by the introduction of every new case, the disease almost invariably commenced to prevail among the sick and convalescents of other diseases, but in isolated hospitals, such as the government has wisely provided

in the Lower Bay, offering every facility for disinfection of *dejecta*, and insulation from the community.

2. *By a Quarantine of Well Persons exposed to Cholera, for a Period longer than the Time of Incubation.*—With the experience of one hundred and fifty-two sick, received from among two hundred and sixty-five apparently well passengers, from one cholera-vessel, within ten days subsequent to their landing, the wisdom of this measure cannot be questioned. If we acknowledge the fact that one cholera-patient in any stage of the disease may become the source of infection in a hamlet or town, the necessity of quarantine of persons exposed to the disease is self-evident.

3. *By Disinfection and Cleansing of the Apartments of Cholera-sick and Well Passengers exposed to Cholera.*—Thorough cleansing, washing with carbolic acid, fumigation with chlorine or bromine, seem to have been, in every instance, sufficient to disinfect the steerage-room or vessel, in which cholera-sick arrived; and not an instance is on record which would lead us to question the efficiency of this method. The quarantine of merchandise may be safely reduced to the quarantine of merchandise soiled by the dejections of cholera-patients and to personal effects.

Variola.

An efficient quarantine established by preventing the intercourse of the community with variola, and the personal eliminations of the disease. The persons are either the patients already manifesting the presence of the disease in the peculiar eruption, or persons exposed to the virus, in which the disease has not yet made its appearance. The eliminations of the disease are its products—the scab in its progress to maturity and the more ethereal eliminations impregnating the clothing and other personal effects of the diseased within a certain distance. The quarantine of small-pox consists, therefore—

1. *In the Quarantine of the Sick.*
2. *The Quarantine of the Well, exposed to the Disease.*
3. *Quarantine of Personal Effects.*

1. *The Quarantine of the Sick.*—The disease has become so domesticated, that isolated small-pox hospitals are among the permanent institutions of our country. Small-pox patients are now carried to Blackwell's Island, and the only objection to it is the great distance from the established quarantine.

2. *The Quarantine of the Well.*—In the determination of the quarantine of well passengers, exposed to the virus of small-pox, the extent and source of the disease, as well as the mode of isolation of the sick, must necessarily guide the health-officer in granting pratique. If occurring as an isolated case, only traceable to the port of clearance, *vaccination* of the passengers and immediate pratique is all that is desirable. When, however, a number of cases have occurred, the port of clearance not being the source of infection, but the sick on board of the vessel itself, thorough vaccination, if possible by fresh lymph, and revaccination of all that have not taken the vaccination after four days' quarantine, are indispensable; and, as vaccination only requires three days to manifest its success, entire and absolute immunity from the disease, pratique may then be granted, with the assurance that no new cases of variola will develop themselves.

3. *The Quarantine of Personal Effects.*—The disinfection of clothing, bedding, or personal effects, impregnated by the eliminations of small-pox patients, may be effected by a continued high temperature, interment or fumigation, or by over twenty-four hours' exposure and ventilation on a warm summer's day. Thirty minutes' exposure of the scab of the small-pox patient—the most concentrated form of personal eliminations—to a temperature of 130° and upward, will destroy its virus as effectually as three days' burial in dry clay—a process sufficient to render the virus perfectly inert.

I may add here that vaccination of the small-pox patient, in the first day or two of the appearance of the eruption, will produce an abortive case, and shorten the term of the disease to the thirteen days required for maturity of vaccination. The vessel itself requires no further quarantine than is required to wash and clean the rooms occupied by the sick, and

by fumigation, entirely destroying the germ of future infection. A scab from a patient exposed to the fumes of nitrous acid will only become inert after continued exposure; while chlorine will only produce its effect in its concentrated form. Fumigation in small-pox is, therefore, to say the least, a questionable expedient, and certainly much less reliable than high temperature.

Typhus.

The records of the Quarantine Hospital show that cases of typhus were admitted during almost every year, from its opening, in 1798, to the present day, almost invariably occurring after long passages on board of crowded vessels, with insufficient and unwholesome food. But it was not until after the famine in Ireland, in 1847, that the disease really became epidemic. From that year until 1853 over twenty thousand cases were admitted and treated in the Marine Hospital, crowding the buildings to overflowing, and necessitating the temporary occupation of the public stores for hospital purposes. These were four stories high, large and massive built, with ceilings not more than nine feet high. The mortality in these was, however, so fearful that they had to be abandoned, and temporary shanties erected, thereby diminishing the percentage of deaths to nearly one-half. Since 1853, the number of cases has been steadily decreasing, until in the last few years comparatively few well-authenticated cases have occurred, either on shipboard or in the Emigrant Hospital. With increased facilities of intercourse and commerce, of railroads and steamboats, famine has become wellnigh an impossibility, and typhus, the legitimate offspring of inadequate nourishment and bad ventilation, has ceased its ravages in Ireland, its principal habitat. With the advent of ocean-steamers and quick passages, it is scarcely possible that we shall ever have any considerable number of typhus-patients to provide for, or that there will be occasion for quarantine regulations to prevent its introduction. Typhus can originate as an epidemic only from grand national disasters, and, as already mentioned, it follows

in the wake of famine and destitution, and is communicable, contagious, and infectious only by personal eliminations of the sick poisoning the immediate atmosphere, and rendering all within its precinct amenable to infection. The more concentrated the poison, by reason of the great number of the sick, and the less the number of cubic feet of air allowed to each individual, the more intense the poison, and the greater the danger; hence the terrible mortality in the public stores already mentioned, where not merely fifty per cent. of its inmates, but also of the nurses and attendants who entered them, were among the victims. To the same cause may also be attributed the virulence of the disease on board of emigrant-vessels crowded with passengers, imperfectly fed, and with less than the requisite amount of air to maintain a healthy condition.

The quarantine of typhus, therefore, is simply isolation of the infected; and the present provision at Ward's Island is probably as good as can be provided, as no possible danger could accrue to the city in their transportation, or to the other sick of the hospital, if any number of typhus patients should be provided for in isolated buildings on the island. Not only those sick of typhus, but all who have been exposed to, and therefore subject to the disease within the ordinarily accepted period of incubation, should be immediately removed; and I know of no place better adapted for the reception of such passengers than the extensive fields of Ward's Island, where, during the winter season, they can be placed in well-ventilated houses; but in summer, if possible, they should live in tents. An instance of a quarantine of this kind we had a few years since in Perth Amboy, where the passengers of a badly-infected vessel were disembarked, quarantined in open tents, and not a single case occurred after their disembarkation. Indeed, I hold the practice of retaining passengers on board of vessels on which typhus had prevailed as most reprehensible; for, although the sick may be removed, and every precaution taken to prevent the further progress of the disease, it cannot thus be arrested. In the instance of the Cynosure we

have the most painful illustration of this. Not only did further cases occur after debarkation at quarantine, but valuable lives were lost, and the entire depot at Castle Garden was endangered by this nefarious practice. The quarantine of *vessels* after the debarkation of the sick and well is only requisite for the time necessary to thoroughly cleanse and fumigate them; and here the fumigation by chlorine gas, generated from a mixture of the black oxide of manganese, salt, sulphuric acid, and water, is certainly the most efficacious. *Merchandise* from vessels with typhus on board can *never* be the subject of quarantine; for there is no instance on record in which it was, or could possibly have been, the morbific agent of the disease.

In speaking of the quarantine regulations, I have treated the subject as my long experience and familiarity with the practical workings of quarantine demand; and, in presuming to make suggestions before the Academy of Medicine, I invite their deliberations to questions which can only be decided by men of science, and thoroughly familiar with the mode of propagation of each disease. The community looks to the profession as its legitimate guardians, and the merchant for relief from exactions and impositions.

Stated Meeting, January 18, 1872, Dr. E. R. Peaslee, President, in the Chair.

The President announced the reception of the *Edinburgh Medical Journal*, for January, 1872, and the "Centenary Address," delivered before the Society of the New York Hospital, July 24, 1871, as contributions to the library.

Dr. Benjamin Howard read a paper on the "Management of Enlargement of the Prostate."

The Academy then adjourned.

Stated Meeting, February 15, 1872, Dr. E. R. Peaslee, President, in the Chair.

The Secretary reported the death of Prof. Charles A. Lee, a non-Resident Fellow of the Academy, at Peekskill, on the

14th inst. The subject of "Quarantine Regulations" was discussed by Drs. Bell, Harris, J. C. Peters, Walser, and Roberts.

The Academy then adjourned.

Stated Meeting, March 7, 1872, Dr. E. R. Peaslee, President, in the Chair.

Drs. John W. Robie and Rudolf Tauszky were elected Resident Fellows.

The President announced the reception of a pamphlet, entitled "Frattura Artificiale," by Dr. Francesco Rizzoli, of Bologna, Italy, as a contribution to the library.

The President then announced the paper for the evening, entitled—

THE MODERN OPERATION FOR STRABISMUS.

By E. G. LORING, M. D.

Mr. President and Fellows of the Academy:

An exception might certainly be taken to the epithet of modern being applied to an operation which has been in vogue for a dozen years or more. But, notwithstanding that the main outlines of the operation are the same which marked it in its earlier days, after the labors of Bonnet, Graefe, and Donders, had become known, still, little by little, improvements have been added to it, and so quietly, as to almost pass unnoticed, till the chapter of strabismus, in its broadest and most comprehensive sense, is a very different thing from what figures under that name in the books which ordinarily meet the eye of the general practitioner, or even in the majority of the more ambitious treatises, which are usually prepared with particular reference to those who make the diseases of the eye a special study.

It is, therefore, with the hope of presenting to those of you, who have not time to search through the various monographs for more detailed information, a slight sketch of this important

subject as it stands at present among ophthalmologists, that I now offer these remarks, and with the hope of pointing out some of the most important considerations, both as to cause and effect, in a subject in regard to which the general practitioner must oftentimes be consulted prior, if not in preference to, the oculist; and, although my remarks will apply most particularly to the operation itself, there are some preliminary considerations in regard to the etiology of the complaint, to which I should like to briefly allude.

It was ascertained, by the labors of Donders and others, that strabismus occurs in eighty to eighty-five per cent. in hypermetropic eyes, or eyes which were congenitally deficient in focalizing power.

Now, without wishing to impugn this statement in the slightest degree, it has nevertheless always struck me that an exaggerated importance has been given, not only by Donders himself, but by all others, in regard to the share which the shortened antero-posterior axis really plays in the production of squint; for it must be borne in mind that emmetropia, or a perfectly normal eye, is a rather poetical condition; and trifling deviations from the usual standard, though sufficient to justify a classification of eyes in regard to refraction, cannot be sufficient to be the active cause of strabismus. Thus, it is hardly fair to suppose that a young child who had strabismus of some three or four lines, and whose total deviation from the normal standard amounts to a convex glass of one-fortieth or one-fiftieth, squints, because it is unable to neutralize this very trifling defect by the action of the ciliary muscle. This attributing the production of strabismus almost entirely to the existence of congenital far-sightedness, no matter how trifling it may be, has led Donders and others to criticise somewhat unjustly the statements made by general practitioners, backed up by those of nurses and mothers, that the squint in a given case had been caused by an attack of measles, whooping-cough, scarlet fever, or any of the thousand-and-one depressing maladies which are incident to childhood. Donders has averred that, if these cases are critically examined, it will almost inva-

riably be found that hypermetropia is at the bottom of the evil, and less renowned and less discriminating practitioners, following in his footsteps, have gravely, if not superciliously, waived away the convictions of the parent and family physician with the calm assurance that the child was born, if not with the fruits, at least with the seeds of the malady, and the cause to which they had attributed the evil had nothing to do with it. Now, I myself have frequently seen strabismus produced in children immediately after some sickness, where there was no error in the optical condition, and pass away as soon as the strength of the patient was restored, from which I have been forced to the conclusion that, wherever and from whatever cause the combined force of the ciliary muscle and interni recti is greater than that of the externi, there also strabismus is apt to occur, no matter what is the optical condition of the eye; and that such a state of affairs will take place more frequently in far-sighted eyes than in others, follows as a matter of course. But, the point which I wish to emphasize is, that a transient reduction in nervous force, with over-exertion at such a time, may produce in emmetropic, or even in slightly myopic eyes, precisely the same factors favorable to the production of strabismus which exist through congenital structure in a hypermetropic eye. In other words, that such eyes, under the debilitating influence of disease, do become, practically speaking, for the time being, hypermetropic eyes, and are forced, from a reduction in focalizing power, precisely like hypermetropic eyes, to use a disproportionately large amount of accommodation with moderate degrees of convergence. So, too, a slightly hypermetropic eye, which, while the general health was undisturbed, would never manifest the slightest tendency to turn in, may become, when reduced in strength, equivalent to a hypermetropic eye of great degree, with its accompanying tendency to become crossed; and the lesson which I would draw from this is, that the general practitioner should be particularly on his guard not to permit his young patients, in the earlier days of convalescence, the indiscriminate use of their eyes, especially when confined to the bed, on books and toys, and other occu-

pations, which are often carried on with an expenditure of nervous force which, if applied in their weakened condition to any other organ, would end in its utter overthrow.

We will pass now to a short consideration of the treatment of the deformity, and, as we have seen that convergent squint usually arises in eyes which are wanting in focalizing power, the first indication should be to restore this deficiency. Optical therapeutics have now become almost a science in themselves, and it would be impossible even to approach the discussion of such a subject in the short time allotted to us, but it may be put down as a rule that, the sooner and more closely an emmetropic or abnormal eye is reduced to a normal one the better, both as to the prevention of the occurrence of the malady and its removal after it has once occurred. This, with a hypermetropic eye, is, as you are aware, done by the use of the proper convex, with a myopic eye the suitable concave glasses.

We come now to a consideration of the operation itself, which might be considered so simple as to need but little explanation. Still this simplicity is only obtained by a strict attention to certain rules, the observation of which is not as common, even among ophthalmologists themselves, as I think should be the case. The first step is, as you are aware, to pick up the conjunctival and sub-conjunctival tissues lying over the insertion of the tendon to be divided, which should be done, though with no violence, still with enough firmness to insure a not inconsiderable fold of the conjunctiva between the arms of the forceps; for, if this is not done, it will be found that where the opening is made the scissors will glide beneath the conjunctiva alone; and the sub-conjunctival tissue, together with the extension of the capsule of Tenon, will have to be divided separately, which will necessitate a new hold with the forceps. Nor must the first incision of the scissors be, as is often recommended in the books, immediately at the corneal margin, but removed from it about a line, and just in front of the implantation of the tendon. This leaves a narrow line or bridge of conjunctival tissue still attached to the cornea,

which is often of great service in the finishing of the operation, and which will be further dwelt on below. But the principal difficulty which is experienced, by even those who have performed the operation repeatedly, lies in the easy and successful passing of the hook beneath the muscle, so that the insertion shall be plainly exposed, and this difficulty is occasioned by a disregard of the anatomical distribution of the capsule of Tenon. The capsule, as you are aware, starts from the optic foramen, surrounds the optic nerve, and, then opening out, forms a cup-like cavity for the posterior half of the eye, in which the eye revolves. Anterior to the equator, however, the capsule is pierced by the muscles, which pass directly through it, to be afterward implanted in the sclera. At the place of perforation of the muscles, the capsule of Tenon sends out reflections, which, ranging not only with the convective tissue-sheath, extend backward toward the origin of the muscle, but also forward toward the insertion where the capsule becomes mingled with the conjunctiva. In the anterior part of the eye, adhesions also take place between the capsule and sclera, by means of filamentous bands extending from the former one to the latter. The effect of all this is, to form, so far as the anterior part of the eye is concerned, a sort of sheath or sleeve in which the muscle is inclosed. Now, the common way of making the opening into the conjunctiva is, to continue with the scissors directly back, in a line with the belly of the muscle, till it is judged that a sufficient opening is made, the hook is then inserted, but the operator is often surprised to find that, after one or two attempts, it does not glide beneath the tendon. The opening in the conjunctiva is enlarged, a more determined effort is made, and it is found that this time, although the instrument did not pass clearly, as it should, beneath the tendon, the point is covered by some portion of the muscle, which is hastily divided, and the operation is finished as it began, in successive attempts to fish up and divide the remaining attachments of the tendon; or it may be in another case that, after a little violence, something is felt to give way, and the hook to glide with the utmost

ease beneath the muscle, without the operator pausing to think what was the cause of the difficulty in the first place, or the success in the latter. This was evidently due to the fact that the hook, in being rotated to pass behind the muscle, came in contact with the wall formed by the capsule, and either could go no farther, or, by carrying the capsule with it, proceeded far enough to just engage the muscle so as to hook it up by the centre as it were, without fully passing behind it. Now, the density of the capsule varies in different cases, and sometimes, with using a little force, we are able to push the hook through it, and then, of course, it is plain sailing. Sometimes, however, the capsule is so strong that the eye is carried before the hook, and made to rotate in large circles in the orbit, and the difficulty of getting beneath the tendon increased. I think, however, that all these difficulties can be almost invariably avoided, if, instead of, after getting through the conjunctiva, we dissect directly backward, parallel with the belly of the muscle, we carry the point of the scissors in a bold, free manner obliquely across the insertion of the muscle, almost at right angles to direction of the muscle, till we get into the loose tissue beyond; by this little manœuvre we make a small hole directly through the conjunctiva and sub-conjunctival tissue, and at the same time through the capsule and the processes which attach the muscle to the sclera. If then, in withdrawing the scissors, we gently raise the edge of the wound, the hook passes with the greatest ease beyond the horizontal edge of the muscle, and falls at once, by the well-known movement of rotation, directly beneath the muscle, so that when the conjunctiva is gently drawn over the distal end of the hook, the tendon will be exposed for its entire length. This being the case, it only remains to divide it, and, in doing this, it is an essential point to never begin at that part of the hook which is nearest the shank, but at the distal end, and to gently raise the point of the hook as it is gradually dissected out, so as to prevent it from disengaging itself from the muscle till the tendon is properly divided. These are apparently small points, but I am convinced, by experience, that a careful attention to

them greatly facilitates the rapid and successful performance of the operation.

It was formerly thought an advantage to make the opening in the conjunctiva as small as possible, and to make the section of the tendon as sub-conjunctival as circumstances would permit. But the dividing the muscle in this way is rather a *tour de force* than any practical advantage, even if it could always be done with certainty, which is by no means the case, and then the original wound has to be enlarged. It is better to make a sufficient opening at the start, and to close it at the end of the operation by bringing the edges of the wound together in the most delicate manner, with the finest possible silk, the ends of the section being cut as close as possible. This gentle closure of the wound produces no inflammatory reaction, and the suture comes away of itself, on account of its fineness, after a day or two, almost unperceived.

We come now to the consideration of the later improvements, which are in themselves of a good deal of importance, and often form, indeed, the principal features in the operation. These are the means which we have in our power of increasing or diminishing the effect of the tenotomy. This is done by the use of sutures. We will take first the suture which is employed to increase the effect of the operation. The most effectual one, of which I am aware, is that which was first brought into general notice by Dr. Knapp, a few years ago. The manner in which it is applied will be better understood by a diagram, which we will suppose represents a pair of eyes, in which there is a very pronounced squint of the right eye, say of five, or even six lines. We will suppose that the patient habitually squints with this eye, and that the excursion outward of this eye, though still good, is somewhat limited, and can only be attained to a full extent by excessive convergence of the other eye. Under these circumstances we have good reason to believe that the externus, from being constantly on the stretch, has lost some of its elasticity, and will not, by forcibly retracting, draw the eye outward after its antagonist has been cut. We will suppose that the internus

has been divided in the usual way, and that the effect, from the causes just mentioned, is very small, so small, indeed, that there is danger of the tendon reapplying itself at or near the seat of its original implantation. It is manifest that we shall have gained little or nothing by the operation, and it is under these circumstances that the suture renders incalculable services. Dr. Knapp's description of the method is as follows:

After a thorough tenotomy of the internus, a small fold of the conjunctiva of one or two lines is picked up close to the outer edge of the cornea. A curved needle, armed with pearl silk, is pushed through it, and the needle then carried through the external commissure from within outward, and the cornea is made to approach the outer angle of the eye to the required amount by drawing on the suture, which is then tied down upon the skin. By this means, a fixed position is obtained for an eye, which otherwise would have a tendency to turn in during the time that the tendon is gaining its new attachment, which latter, as the eye is forcibly turned out, must lie farther back than otherwise would be the case, and the effect is consequently much increased. I generally seek to obtain about a line of divergence of the operated eye immediately after the operation. The suture should be left in one or two days. It very often comes away of itself, on the second day. If, however, the squint is a divergent, instead of convergent, the suture, though still acting on the same principle, is applied a little differently.

A thorough tenotomy of the externus having been made, a suture is armed with two needles. The point of the first needle is entered close to the inner side of the cornea, and one and one-half line above the horizontal meridian, and, after having been run through the conjunctiva from above downward, is again brought out in the horizontal meridian. The second needle is carried in precisely the same way from below upward, and likewise brought out in the horizontal meridian. The two ends of the suture are then drawn so that the middle of the suture lies in close proximity with the globe. This prevents the conjunctiva from being torn through

by the subsequent tension. The needles are then carried in close proximity to each other through the commissure of the lid, the entrance of the needle being just above the caruncle, and their point of exit two lines from this in the integument of the nose. By drawing both ends of the suture, the inner edge of the cornea is made to approach the caruncle to an amount proportionate to the effect desired. The knot is then tied upon the skin of the nose, and the eye thus secured in its position till the tendon of the divided externus has acquired its new attachment. The amount of adduction produced by the suture should be such that about one and one-half line of convergence should be produced, since experience shows that, when this amount is not present immediately after the operation, a certain degree of divergence will finally result. The suture is to be removed on the day after the operation.

Graefe's method of increasing the effect of a tenotomy is as follows: After the tenotomy of the internus has been thoroughly performed, a fold of the conjunctiva is lifted off from the episcleral tissue with the forceps, close to the outer canthus. A curved needle is then run horizontally through this fold into the sub-conjunctival space, and made to glide (the forceps being removed to the upper and inner border of the cornea) beneath the membrane, till a portion of this, equal to three, four, or five lines, has been included, when the point is again brought out to its place of exit, varying according to the length of its sub-conjunctival passage, its direction being always from the commissure to the upper and inner corneal border, or it may be even to a point above the vertex of the cornea. The suture is then tightly drawn, and tied. By this manœuvre the eye is rolled outward to the breadth of the inclosed tissue, and the limitation in the movement of the eye inward is much increased. In this way Graefe says we are enabled to obtain, in excessive degrees of convergent strabismus, any correction desirable. The suture must be left in at least two and one-half days.

The suture is employed in precisely the same manner in divergent strabismus, only it starts from the inner canthus,

near the caruncle. Graefe says that this method has been of signal service to him in the higher grades of strabismus, and that since its adoption he has been far more reserved in the use of the advancement of the muscle. Indeed, he asserts that he scarcely ever performs this latter now on account of divergence, but only on account of marked limitation of the mobility inward.

Such being the means by which the effect of tenotomy is increased, those by which the effect is diminished remain to be spoken of. This is still done by the use of the suture, and their mode of application will be made clear, I think, upon the board. We will suppose that in a case of convergent squint we have already divided the internus, and that the effect obtained has been excessive, and there is fear that the eye will go the other way, or at least there will be an insufficiency of converging power, which may impair the use of the eyes for any length of time upon close objects. The object is to lessen the effect of the original tenotomy, and this is done by the suture, the effect of which is graduated in the following ways: If we have reason to believe that the effect of the tenotomy is only a little more than that which we wished originally to obtain, we simply bring the edges of the wound together in a vertical direction; this, of course, prevents the muscle from retracting as far as it otherwise would, and the effect of the operation is somewhat limited. We can increase this again by including more or less conjunctiva within the stitch. If we want to limit it to a greater degree, then, instead of passing the needles vertically, we run them obliquely. This of course draws the muscle still more forward, and the effect can be still further increased by including more of the conjuctiva between the stitch. Or, if, lastly, we wish to reduce the effect to a very considerable degree, we then pass the needle in a directly horizontal direction, taking up more or less of the conjunctiva, according to circumstances.

It has been asserted with a good deal of force, especially by Von Graefe, that the controlling influence of these sutures is such that we can, if the operation has been skillfully per-

formed, invariably get any effect which we may desire. While admitting to the fullest extent their great usefulness, and the very important *rôle* they play in enabling us to increase and diminish in a general way the effect of a tenotomy, candor compels us to say that they have not in our own hands acted, nor can we understand, from the very nature of things, how they can possibly in anybody's act, with that mathematical precision which the great master has claimed for them; and we join with Wecker in expressing a regret that one who has added so much to the practical researches of Bonnet should, through an excess of zeal in perfecting tenotomy, be led to believe the operation for strabismus and the graduating of its effects susceptible of a precision which does not belong to them. I know of no absolute rules which can be laid down as a guide, either as to the immediate or permanent effect of a tenotomy. Still there are three things a careful study of which should never be neglected: 1. The condition of the externi, and the extent of motion outward; 2. The refraction of the eye; and, 3. The effect of the accommodation. If, for example, the abduction of the eyes is limited, or only obtained by a vacillating motion, as soon as or shortly after the squinting eye has passed the median line, it will be safe to predict that a greater setting back of the muscle will be required, and the effect of the operation will have to be increased by dividing both interni, where, under ordinary circumstances, we should divide only one, or, what is better, by the use of the suture; and we may be sure of this, if, after the patient has been thoroughly narcotized, we find, before we begin the operation, a divergence of the eyes. I have been so often deceived by this amount of divergence, when patients are under the influence of an anæsthetic, especially just after the muscle has been divided, that I pay no attention to it where I have once convinced myself that there is some weakness of the externi, but base my calculations entirely on the examinations made before the anæsthetic had been given, and this especially in regard to the suture.

Again, we may put it down as a rule (but as a rule that has

many exceptions) that the greater the amount of hypermetropia the greater the amount of operating we may do without the fear of obtaining too great an effect. And here I would again insist upon the necessity of reducing the eye, before resorting to repeated operations, by the means of glasses, to as near a condition of a normal eye as possible.

Finally, if the eye swings in suddenly under the influence of the accommodation, so that the squint is disproportionately larger for the near than for the distance, we may assume that the energy of the externi is reduced, and a greater freedom allowed in operating than where the reverse is the case; that is to say, where the squint neutralizes itself somewhat rapidly, when we approach an object toward the eye so that at moderate distances, twelve to sixteen inches, the two eyes seem to be directed to the object. Starting, then, with the assumption that, in an ordinary case of concomitant squint, we get from two to three lines as the effect of an operation, the above rules may be of some service in some of the more common deviations from the general run of cases. But, for more than this, I should not like to be responsible. I would not wish, by this, to have you infer that I consider the operation for strabismus a work of chance, for I sincerely believe that the more fully we understand and the closer we follow the optical and dynamical laws which govern the condition under consideration, the more successful we shall be. Still I cannot yet believe, however hopeful I may be for the future, in that mathematical exactness which is at present often claimed for it. The only rule which can be conscientiously followed is to estimate by careful study as closely as we can the probable result, and then to operate till success is obtained.

Some eight or ten hours after the operation, or when the narcosis has passed entirely off, the patient should be seen again, when another examination should be made, and this should be done as Graefe has shown—not as is usually the case, directly in front of the patient, that is, in the median line, but toward the temporal side of the eye, which has been operated on, the object being at about fifteen feet from the

patient and at about fifteen degrees toward the temporal side. This, of course, throws the strain off the divided muscle, and the insufficiency which follows as the immediate effect of the section is reduced, and a more correct estimate of what will be the permanent effect is arrived at. Thus, if the right internus has been divided, the patient should look to the right. I prefer, however, to make my examination in both places, that is, directly in front, and then toward the temporal side. As a rule, when on seeing the patient I find that less effect has been obtained than was estimated, I content myself with leaving things as they are, preferring to operate on the other eye, using the experience gained as to the effect on the first eye as a guide to the second operation. But, whenever my estimation of the antagonistic force of the externi has been so much out of the way that very little or no effect has been produced, and the muscle is in danger of reattaching itself at or very near the place of its original implantation, I never hesitate to interfere, and in this case I always use the suture described by Dr. Knapp, never that by Graefe, the effect of which varies, as he himself allows, in every case, according to the different degrees of the elasticity of the conjunctiva. If the patient is an adult, and possesses a moderate degree of nerve, an anæsthetic may be dispensed with. The wound can be reopened and the new and yielding attachments readily severed with the strabismus-hook. The suture is then to be placed at the outer edge of the cornea, and the eye swung out to the proper amount in the manner just described on the board. In children it is better to always use an anæsthetic.

If, however, on the first visit after the operation, there is good reason to fear that too much effect has been obtained, the matter is much more serious, and should be remedied as soon as possible. It is, however, extremely difficult to tell, or at least to lay down any exact rules by which it can be safely judged that too much effect has been obtained. Here it is that the experienced eye has so great an advantage, for any one who has seen and studied a large number of cases can often tell at a glance that too much effect has been gained,

while it would puzzle him to say exactly how he was aware of it. Still, the following method of examination has been of service to me in a general way, and as such I now offer it.

If I find, twelve hours after the operation, that there is a line of divergence, when the patient looks at an object at say fifteen feet, I look upon it not only as no detriment, but a positive advantage for the permanent effect, provided the following conditions exist with it: 1. That the object can be gradually approached toward the patient in the median line without the divergence in the operated eye increasing to any perceptible degree till the object gets inside of 12″ or so. If, on the contrary, the divergence gradually increases, or even remains the same till within 10 or 12″ of the eye, and then the eye suddenly flies out, or even begins to waver, we have cause for apprehension, and the farther that such a condition takes place from the eye the greater the dread. Again, if we have a line or even more of divergence in the median line for the distance, and this disappears gradually as we carry the object toward the temporal side of the eye operated upon, till the object lies 15° toward that side, we have little dread that the effect is excessive, especially, as is usually the case, the object can be carried close up to the eye without the operated eye yielding.

Should, however, the conditions of these two tests not be fulfilled, it would be on the safe side to limit the effect of the operations by the use of the suture, and the amount of the limitation should be graduated by the degree of the divergence and the distance at which it took place. The proper amount, or its approximation, can, I think, only be learned by experience, together with a careful consideration as to the effects to be produced by the existing refraction and accommodation.

The elaborate rules laid down by Von Graefe in regard to dynamic squint, or insufficiency, are not, of course, applicable here, as requisite sharpness of binocular vision very rarely if ever exists to make such tests possible, much less trustworthy. The estimation must be made by the operator's eye, not by means of prisms.

The indications for the operation for strabismus are rapidly

becoming better known, so that many more cases come to operation in early life than formerly. Still, there is a lingering desire, especially on the part of general practitioners, to advise the parents to wait and see if the child will not grow out of it, and to assert that cross-eye often gets well of itself, and if it does not it will be time enough to talk of an operation when the child has reached ten or twelve years of age. Now, it is true that strabismus does sometimes disappear of itself, but it is an exceedingly rare occurrence; so rare, indeed, as to be the exception necessary to prove the rule that it does not cure itself, and that in the vast majority of cases recourse must be had to an operation. In regard to this there is no diversity of opinion. But in regard to the time of life at which the operation should be performed there is, I am sorry to say, much less unanimity, and the general profession have certainly the best of excuses, for the Fabian policy which they recommend, in the authority of some of the greatest writers on this subject. Thus Donders advises the use of atropine in young children upon whom it is as yet unadvisable to operate. And Stellwag deliberately says that "it is better to postpone the operation till the beginning of puberty, if the strabismus be seen in childhood; by thus delaying, the conditions are fulfilled under which periodical strabismus is removed, and the patient may, as it were, grow out of a squint which has already become permanent."

Notwithstanding the weight of such authority, I am decidedly of the opinion that the cases in which the operation should be deferred to a comparatively remote period are few in proportion to those that should be operated upon as soon as seen. And the chief points which should influence our decision, either *pro* or *con*, are as follows: If the child is very young, say from one to three years, and the squint is concomitant so that the exclusion of the two eyes is equal, and at the same time the squint is bilateral, so that the child fixes sometimes with one eye and sometimes with the other, it will be safe to postpone the operation almost indefinitely, or so long as the conditions described hold good; for under this

state of affairs the normal mobility of the eye and the acuity of vision will be maintained; and this rule should be observed all the more if there be hypermetropia, for if this be of any considerable degree the squint will most likely return after the operation, as the age of the patient will prohibit the correction of the optical defects by glasses.

But squint of this character is very rare in infants and young children, as it usually results, not from an optical defect, as the accommodation in children is not actively employed, but from some paresis of the muscles, so that the squint is confined to one eye, and vision is entirely performed by the other, or so seldom by the squinting eye that this is practically excluded from the visual act. If such eyes are left to themselves, two things almost invariably occur: 1. The vision deteriorates so that the eye in a short time becomes useless; 2. The externus, from constantly being put upon the stretch, loses all its vitality, and the mobility of the eye outward is so reduced that the eye after a while gets the appearance of being, if I may so express myself, broadside on, producing, as the child grows up, hideous deformity, and one which later in life requires almost an endless number of operations to remove. If, on the other hand, we operate early, when the weakened muscle still has a share of its vitality left, we can do no possible harm, and we stand the chance of doing infinite good, either by curing the deformity at once, or at least of turning what was a unilateral squint, with exclusive use of one eye, into a bilateral squint, with the alternate use of the two eyes, thus offering a much increased chance for the preservation of the vision, and a much better prospect of success from a future operation when the muscles shall have more fully recovered their tone. In these cases it is almost always necessary to use the suture. Between the age of four and six, we begin to get the common concomitant strabismus, dependent on either a congenital defect, that is, hypermetropia, or, what amounts to the same thing, an imitation of this through the temporary reduction of the accommodation power by some debilitating disease. In this latter condition, that is, when the eye is emmetropic,

or even slightly hypermetropic, I never hesitate to operate: 1. With the hope of restoring binocular vision; and, 2. Even if this fails, of removing the deformity, for if this is once done there is little probability of its returning, since, as there is very little or no defect in the refraction, there is no exciting cause to occasion its return. After the age of seven or eight years, I always operate, and insist, if the error of refraction is sufficient to warrant it, that glasses must be worn.

Thus it will be seen, if these views are correct, that the latitude of operative interference is largely increased, at least in regard to the time of operating, in comparison with what is usually held to be sound practice.

There is one other condition of squint in which the general opinion seems to be adverse to operating, and that is the strabismus, of a paralytic nature, occurring in adults. This disinclination to operate in these cases evidently had its origin in two sound objections: 1. An unwillingness to subject the patient to the pain and annoyance of an operation when his strabismus is either accompanied by, or supposed to be the forerunner of, grave central trouble; 2. From a conviction that it was useless to operate on a muscle when the antagonist of that muscle was either partially or wholly paralyzed, as no effect would be gained.

In regard to the first of these objections, it is no doubt true that any operative interference would be contraindicated if central trouble was shown to exist, but there are very many cases in which this does not manifest itself in the slightest degree, either at the time of the attack or later, or even, if it does in the beginning, fades away in a short time, while the strabismus, with its annoying diplopia, remains. Under these conditions, it seems to me that the indications for the operation are as strong as, if not stronger than, in any other form of strabismus. And, that they are so, I think will be more forcibly and more briefly shown by an example which is neither a rare nor an imaginary one.

A man in the prime of life, after some exposure, or an unusually hard day's work, or even after ordinary routine of the

day, goes to bed, as he thinks, perfectly well, and is astonished on waking up in the morning to find that he sees double. He immediately consults his family physician, who at once sees where the trouble lies, and, after a careful examination of his general condition, pronounces his eyes sound, prescribes rest, absence from excitement and all abuse, whether of spirit or the flesh, and gives iodide of potash in increasing doses. This may or may not have some effect; usually it does not, and the changes are rung on the bromides and iodides, tonics, and electricity; still the double vision persists, and what at first was a simple annoyance, now becomes an actual suffering. There is a constant dragging sensation about the side of the head, with pain and tension in the forehead. The patient begins to have a difficulty in getting about, or even, at times, staggers or misses his footing. It is an exertion for him to think, and he begins to fear that he is getting light-headed, or that something is the matter with his brain, and it is in vain that his physician or an expert assures him to the contrary. To his suggestion that, as his eye is turned, benefit might be obtained from an operation, it is answered that, as his trouble is of a paralytic nature, it would be worse than useless; that things must be left to take their own course; and, if this advice is taken, the diplopia generally fades away in a year or so, but the dragging, wearying, aching sensations in and about the eye usually remain, either as a constant companion, or to be called forth by the slightest over-exertion or excitement.

If, on the other hand, the patient, after a longer or shorter interval, seeks the advice of the trained ophthalmologist, a minute examination is made, and I am afraid, as a usual thing, precisely the same advice is given—exceptions to this rule, though I am well aware they do exist, being few and far between. Sometimes, however, the oculist consulted, seeing that very little harm can result, boldly makes up his mind to run the chance of doing some good, and is often rewarded for his courage, either by the cure or relief of his patient.

The second objection, that we get no effect from the operation when the difficulty is a paralytic one, fades entirely away,

since by the use of the suture, or by advancement of the muscle, we can get any effect we desire, no matter what the cause of the strabismus is.

The treatment of paralytic strabismus is one of the most difficult problems that the ophthalmic surgeon has to deal with, and the differential diagnosis as to what muscles are affected in a given case as laid down in the books, and upon which we are made to believe our opinions should rest, is a thing which may well appall the boldest. So far as operating is concerned, the practical considerations necessary to determine our opinion, at least in the majority of cases, can be reduced to a comparative few. In the first place, the oblique muscles, whether involved or not, are excluded, from the positions of their insertions, from the category, tenotomy of these muscles never having been, so far as I am aware, followed by successful results. Nor am I personally aware of the attempt to cut these tendons ever having been made, though I have heard traditionary reports to that effect. This limits our attention to the four recti-muscles, and in a great majority of cases it is either the internus or externus which is chiefly affected, and oftener the former than the latter. When the superior and inferior are also implicated, it is usually in a secondary degree, and the deviation a trifling one. When, then, a case of simple paralysis, either of the externus or internus presents itself, I do not hesitate to operate, provided it is not of central origin, and provided sufficient time has elapsed to show that therapeutical and hygienic means will not remove the trouble. The only question then is, what operation we shall do, advancement of the paralyzed muscle with a tenotomy of the antagonist, or simply tenotomy of the opposing muscle and the use of the suture? This depends entirely on the degree of the trouble; if the paralysis is complete, and the motion toward the median line is very much limited, then resort must be had to advancement of the paralyzed muscle as laid down in the books. But if the attack is slight, and rather a paresis than a paralysis, or a paralysis improved into paresis, and there is still some motion toward the median line, then a simple te-

notomy with the suture will often produce, as Graefe has pointed out, the most astonishing amount of effect.

We will suppose, for example, that there is a paralysis of the externus, so that, by the unopposed contraction of its antagonist, the internus, a convergent squint is produced of some three, four, or five lines. If, now, we thoroughly separate the internus from its insertion and then forcibly draw out the eye, and maintain it in this position by means of the suture, we do two things: First, by setting back the internus so that it acts under less favorable dynamic conditions, we reduce to a very great degree its aggressive force which was continually keeping its enfeebled antagonist on the stretch, destroying its elasticity and continually canceling its recuperative power; while, secondly, by swinging out the eye and maintaining it in this position we gain two or three days of absolute rest to the weakened muscle, and thus allow its fibres to retract upon themselves, and regain, if not all, at least as much as possible, of their vitality. The effect of this is often surprising, and I have known a paralytic squint, which had resisted for months all the therapeutical means known to modern science, be cured at once by a simple tenotomy. Here, as elsewhere in the body, the most trifling variations will often produce the most startling effects both for good and evil; and the experiment of an operation which, if skillfully applied, can do no harm and may do infinite good, is surely worth the trial, if not in all attacks, in a very great number of these cases.

The fact of other muscles of the ocular group being affected, as, for example, the superior or inferior rectus, or even the obliques, used to be of great weight with me in determining the propriety of an operation, for it was argued that even if the horizontal double vision was removed, the vertical would remain, and the depression, though different, would still be just as annoying to the patient. But, experience has shown me that it is not the actual diplopia which is the chief cause of trouble and annoyance to these patients, but the want of control they have over the eyes, and the subsequent dragging and

aching sensation; and this is particularly noticeable where the strabismus is divergent. I should not, therefore, unless the complications were extended, and of a grave character, hesitate to operate to remove the lateral strabismus, and let vertical diplopia take care of itself. I have known fifteen or twenty degrees of this to disappear shortly after the lateral deviation had been corrected. But, even if this cannot be corrected either by a second operation or by prisms, the double images are usually sunk in the course of five or six months, and the patient passes from an improved condition from the day of the operation into what is ultimately a very comfortable one.

As before remarked, very few of these cases come to operation, and, the great mistake which usually takes place in those that do is, that they are operated on too late, when secondary contraction of the antagonist has taken place and the weakened muscle has lost its vitality, and then an advancement has to be performed, and not a tenotomy with a suture.

A short time after the paralytic attack, there is generally an effort on the part of Nature to reëstablish the coördinating movements of the eyes, provided the cause is not a central one, and it is during this period that the operation should be performed, and although cases vary and no rule can be laid down, it is usually best not to wait longer than two months after the original attack.

I would say, in conclusion, Mr. President, that I am rather unpleasantly conscious of the fact that the title of my paper has proved to be a misnomer; for, instead of being, as I intended, a sketch of the operation for strabismus, as it stands at present it is only an outline of a small part of it. This has been caused by the vastness of my subject, which has compelled me to leave out much that I wished to write, and to discard much I had already written; and, as it is, I cannot help expressing the fear that my remarks have been both too extended and of too simple a nature for the dignity of the rostrum from which they have been delivered—certainly they have for the critical acumen of many of my hearers; still,

they are some of the points of practical importance over which I myself have thought a great deal, and I have believed that they might be of some use, to those of you whose practice brings you in contact with many children, in answering the question so often put, "Can strabismus be cured?" a question which, though facts do lend it some show of color, is, in the present advanced state of medical art, neither very complimentary to the judgment of the general physician, nor the skill of the ophthalmologist.

STATED MEETING MARCH 21, 1872, DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DRS. T. M. CHEESMAN, JEAN F. CHAUVEAU, W. T. LUSK, J. S. MONELL, CHARLES W. PACKARD, and RUDOLF TAUSZKY, were inaugurated Resident Fellows.

The PRESIDENT announced the receipt of the *Edinburgh Medical Journal* for February and March, 1872, and the *Western Lancet*, vol. i., No. 2, published at San Francisco, California.

The PRESIDENT then announced the paper for the evening, of which the following is an abstract:

ON PELVIC PERITONITIS AND PERI-UTERINE CELLULITIS.

BY JAMES L. BROWN, M. D.

MR. PRESIDENT AND FELLOWS OF THE ACADEMY:

THAT the uterus and its appendages, in common with other parts of the economy, are liable to become the seat of inflammation and abscess, and that such inflammation and abscess are for the most part connected with the puerperal condition, are facts that have been observed and recorded as far back as the history of medical science extends. Beyond this general knowledge of a very obvious disorder, however, it may safely be stated that very little was known of the inflammatory affections of the peri-uterine tissues prior to the beginning of

the present century. Indeed, without much violence to historical accuracy, it may be affirmed that nearly all that is now known with any precision of these ailments has been ascertained within the last thirty years.

Isolated facts had indeed been observed and recorded by different medical writers, as far back as Morgagni, in the early part of the last century, but no systematic study of the peri-uterine inflammations can be said to have been undertaken before the year 1840.

I am aware that Simpson and others claim for the ancients a very considerable and tolerably exact knowledge of these diseases, but I am of the opinion that no person who will take the trouble to read the passage in Ætius, referred to by Simpson, in his article on Pelvic Cellulitis, will find in it a justification of the statement that it contains a full and distinct account, or indeed any account at all, of the affections under consideration. I repeat, then, that neither in ancient nor in modern times, so far as we have any means of knowing, were these affections made the subject of special study until about the year 1840. Of the progress that has been made since that time, accounts more or less complete are to be found in nearly all of the more recent treatises on diseases of women. I shall make no apology, therefore, for omitting historical details that are so readily accessible, and shall confine myself in this paper chiefly to some clinical remarks, based upon the observation of eighty-seven cases, of which I have taken notes, and on impressions received from an equally large number of cases of which no record has been kept.

As it is not my purpose to attempt a full or extended description of these diseases, the subject being altogether too large to be adequately discussed within the limits to which I am necessarily restricted, on an occasion of this kind, but merely to invite attention to certain parts that have seemed to me to be of peculiar interest, or upon which it is very desirable that the light of further clinical experience should be thrown, I shall not confine myself to any systematic treatment of the subject, but deal with it in a somewhat desultory manner,

passing lightly over many interesting topics, and dwelling upon others that are of no more real consequence, perhaps, but which may have proved more interesting in my own experience, or to which I may have given more especial attention.

Of a class of affections that have only so recently been made the subject of investigation, it is not surprising that our knowledge should still be very imperfect; that much difference of opinion among observers should exist on many points; and that what is known about them should be mostly confined to those especially engaged in studying or treating the diseases to which they are allied. Believing that these affections are far more frequent than is generally supposed, being often mistaken for other conditions, and consequently subjected to inappropriate treatment, and believing, moreover, that they are of great practical interest to the general practitioner as well as to those more particularly engaged in the treatment of diseases peculiar to women, it has seemed to me that any contribution tending to throw light upon them, and to lead to a better appreciation of their frequency and importance, might prove both interesting and profitable.

The affections to which I have alluded are variously described by different authors as pelvic abscess, pelvic cellulitis, inflammation of the broad ligaments, inflammation of the uterine appendages, peri-uterine inflammation, peri-metritis, parametritis, peri-uterine cellulitis, peri-uterine phlegmon, pelvic peritonitis, etc. Some of these expressions serve not only to furnish a name, but to indicate a theory. Pelvic abscess, for instance, conveys the idea of the inflammation necessarily resulting in suppuration. The term phlegmon, on the contrary, as used by the French writers, though assigning the seat of the inflammation to the cellular tissue, does not imply any such result. Pelvic cellulitis and pelvic peritonitis give to the disease each a local habitation as well as a name.

The use of the term peri-uterine inflammation indicates a disinclination to be committed to any theory as to the precise seat of the inflammatory affection. We thus find in the very nomenclature of these diseases well-marked indications

of a conflict of opinion as to their seat, their nature, and their tendencies. Passing now from the nomenclature to the definitions given by different writers, we meet with a corresponding want of agreement. Selecting a few from the many, we find, for example, Simpson defines it thus: "Between the layers of the broad ligaments, between the vagina and the rectum, between the iliac muscle and the bone, and, in short, in almost every part of the pelvis, there is an abundance—a great abundance—of cellular tissue; and this disease consists of inflammation, acute or subacute, of this abounding cellular tissue." West speaks of it as an inflammation "which, though most frequently involving the cellular tissue between the folds of the broad ligaments, sometimes attacks that which intervenes between the womb and the adjacent viscera, or extends to that lining the pelvic walls, or even to that which lies between the outer surface of the peritonæum and the abdominal muscles." In another place he adds: "The cellular tissue anywhere in the neighborhood of the womb may be the seat of the mischief, though that contained in the folds of the broad ligament is attacked far more often than the same structure in any other situation."

McClintock makes the following statement: "In regard to its seat, it may affect almost any part of the cellular tissue adjacent to the uterus. Thus I have seen it most frequently in the broad ligament, at least so far as I could judge; in the iliac fossa; between the bladder and the abdominal parietes; between the uterus and the rectum; and in every part of the connective tissue around the neck of the womb."

Graily Hewitt states that, "in its essence, it consists of effusion of morbid products into the space surrounding the uterus and ovaries, and the transformations undergone by these effused matters. . . . The actual seat of the effusion is in most cases the meshes of the cellular tissue surrounding the uterus, between the folds of the broad ligament and extending thence in various directions toward the pelvic walls; but it is probable that in some cases of pelvic inflammation there is an inflammatory condition of the peritonæum itself." Scan-

zoni makes no allusion whatever to inflammation of the pelvic cellular tissue, and devotes but a single page to the description of peri-metritis, which he regards most frequently as a puerperal disease, but which may be occasionally a secondary affection, "accompanying an acute or chronic inflammation of the uterus or the ovaries, or one of the various neoplasmata which are developed in these organs." His description is so very imperfect as to be of very little practical value.

Nonat, who may in some measure be regarded as the French representative of the views most prevalent in England, describes the inflammation under the name peri-uterine phlegmon, thus locating it in the cellular tissue, and divides into four varieties according as it is located on either side of the uterus, and in direct contact with it, or in one of the broad ligaments, or behind the uterus, or in front of it. Though he does not deny the occasional occurrence of pelvic peritonitis, either as a complication of peri-uterine phlegmon or independently of it, he gives no description of it as a distinct affection, and indeed barely alludes to it.

Bernutz, on the contrary, occupies himself chiefly in refuting Nonat's statements concerning peri-uterine phlegmon, and, after proving to his own satisfaction that peri-uterine phlegmon is anatomically impossible, he substitutes for it pelvic peritonitis, and sustains his position by the evidence of several autopsies. I give an abstract of his views in his own words. He says: "The slightest dissection shows that the cellular tissue subjacent to the peritonæum is so thin and scanty that it is impossible to separate the serous from the uterine tissue; and that, consequently, it cannot be the seat of swellings which, according to M. Nonat's observations, attain in the space of a few hours to the size of a hen's-egg. . . . It is anatomically impossible that the tumors which M. Nonat called uterine-latero-phlegmons, and which are quite distinct from the phlegmons of the broad ligament, can be situate in the cellular tissue of the lateral parts of the womb. . . . From all these facts I have concluded that cases of supposed peri-uterine phlegmons ought to be classed with those of partial peritonitis."

Thus we see Bernutz occupying a position directly opposed to that of Nonat, for, while he does not deny the existence of the cellular inflammations, he gives no account of them, and treats only of pelvic peritonitis.

Aran, in adopting the term peri-uterine inflammation, says: "I propose under this name to describe an affection that has by turns received the denomination of inflammation of the lower belly, inflammation of the appendages of the uterus and of the broad ligament, inflammation of the pelvic cellular tissue, and pelvic peritonitis."

Courty also adopts the term peri-uterine inflammation, which he says is preferable to any other name, precisely because it comprehends all the others, and prejudges nothing as to the seat of the malady.

Thus far, then, we find the English writers regarding these diseases as essentially inflammatory affections of the cellular tissue, the peritonæum being occasionally involved. In France we have Nonat regarding cellulitis as the principal affection; Bernutz holding a similar view with regard to peritonitis; and Aran, Courty, and others, grouping both together as peri-uterine inflammation.

It does not appear to have occurred to any one prior to the year 1868 to dissociate these diseases entirely, and to assign to each its proper position as an independent affection, having no more necessary connection with the other than has pleurisy with pneumonia.

Bernutz, it is true, in treating of pelvic peritonitis, differentiates it from phlegmons in the iliac fossa, in which he erroneously includes cellulitis of the broad ligaments; but he is so intent on proving Nonat wrong, that he barely recognizes cellulitis, and gives it no further consideration.

The first distinct enunciation of the fact that pelvic peritonitis and pelvic cellulitis have no necessary connection with each other, that they are entirely distinct affections, and can generally be distinguished from each other clinically, I find in Dr. Thomas's treatise on "Diseases of Women," first published in the beginning of the year 1868.

The two diseases are there described separately each with its own history, etiology, symptoms, and diagnostic character. This distinction once made, it becomes a matter of surprise it was not not made sooner. That partial or local peritonitis is of common occurrence has long been known ; that inflammation and abscess of the cellular tissue is a frequent affection has been equally well known. That they should so long have been confounded in this locality has probably been due to the incompleteness of our observations, the inadequacy of our means of diagnosis, and the infrequency of opportunities to confirm or correct diagnosis by *post-mortem* examinations, these diseases rarely proving fatal.

Nearly a year after the appearance of Dr. Thomas's book, Dr. Mathews Duncan published his monograph on peri-metritis and para-metritis, which is by far the most complete exposition of what is known on this subject that has yet been given. He not only recognizes peritonitis and cellulitis as independent affections, but subdivides each into varieties, of which he treats at some length.

These affections, he says, are "surpassed in importance by none of the diseases of women;" they are "common, painful, and dangerous, both to functional perfection of the parts implicated, and to the life of the sufferer from any of them." By peri-metritis, he means inflammation of the uterine peritonæum; by para-metritis, inflammation of the cellular tissue, in connection with the uterus.

Of this nomenclature I might observe, in passing, that, while the term peri-metritis is unobjectionable, its meaning being self-evident and sufficiently precise, the term para-metritis needs explanation, and is therefore inferior to peri-uterine cellulitis, which he rejects because of its hybrid etymology. The term peri-metric cellulitis might, perhaps, be better than either.

Coming now to the clinical consideration of these diseases, and accepting the fact, which is everywhere admitted, that the inflammation sometimes attacks the peritonæum and at others the cellular tissue, almost the first question that con-

fronts us is this, Have we any means of determining in a given case which of these tissues is the seat of disease? Dr. Thomas says: "They may usually be readily differentiated from each other, and a neglect of the effort at such thorough diagnosis is as reprehensible as a similar want of care in determining between pericarditis and endocarditis." Mathews Duncan is less confident. He says of the diagnosis of these affections from each other: "It is still in a very unsatisfactory state. While the very existence of the various affections is still, in some quarters at least, regarded as an open question, there must surely be at least difficulty of diagnosis to justify doubt." He quotes the tabular statement in which Dr. Thomas contrasts the distinguishing characteristics of the two affections, merely to express his dissent from it both as a whole and in its several parts, yet he adds that we are indebted to Dr. Thomas for even attempting the difficult task of differentiation, and admits that the indications laid down by Dr. Thomas show the kind of indications on which he himself has always relied. For my own part, I am less confident than Dr. Thomas, and less skeptical than Mathews Duncan. In about two-thirds of all the cases that have come under my observation I have thought the diagnosis clear enough to be susceptible of demonstration. The element of doubt, however, has entered very largely into the other third.

I should state that most of my cases have been seen among the out-door patients, coming to the Demilt Dispensary, to the Woman's Hospital, and to the clinique for diseases of women at the College of Physicians and Surgeons, and these were consequently nearly all subacute or chronic cases. The acute cases that I have seen were comparatively few in number, and have occurred mostly in private practice. As I believe that subacute and chronic cases will always form the majority of these affections in general practice, I shall invert the usual order of sequence and speak of them first, beginning with the physical signs on which we must chiefly depend for diagnosis, there being nothing in the symptoms that is at all distinctive. The uterus, as is well known, possesses a considerable degree

of mobility: it may be drawn down to the vaginal outlet; it may be pushed high up in the pelvis, and it may be forced to either side. In women who are not too fat, or who have not an unusual rigidity of the abdominal muscles, that is to say, in the majority of women, the uterus may be distinctly felt through the abdominal walls by making pressure above the pubes with one hand, while counter-pressure is made in the vagina by the index-finger of the other hand. It may thus be included between the two hands, and its size, shape, position, and mobility determined with great accuracy. Now, one of the first and the most constant of the effects of pelvic peritonitis in any of its severer forms is, to impair or abolish its mobility. The finger in the vagina infringes on the uterus, but can neither push it up nor draw it down, nor move it to either side; there is fixation more or less complete. Not only is the uterus immovable, but the whole roof of the vagina is rigid and hard. Whether the finger be carried behind the cervix, or in front, or on either side of it, it meets with a firm resistance of almost cartilaginous density. Occasionally this hardness may not entirely surround the cervix, but in severe cases it nearly always does. In the milder attacks the induration of the pelvic roof may be slight in degree, limited in extent, or entirely absent, but there is always some fixation of the uterus. There is, moreover, almost invariably some displacement of the uterus. It may be flexed backward or forward; it may be anteverted, retroverted, or drawn to either side, but a displacement of some kind almost always exists, and the uterus is held in its abnormal position by adhesions. There is, too, in a certain number of cases, a distinct tumor to be felt, located most commonly behind the cervix, in the posterior *cul-de-sac*, but occasionally on one side and in close proximity to the uterus. This tumor can very rarely be felt through the abdominal walls, and is usually only to be detected in the vagina. These constitute all the physical signs of pelvic peritonitis as it is seen in subacute and chronic cases, excepting those in which there is an encysted collection of serum or pus, and in these we have to add the existence of a fluctuating

tumor to the signs already given. We have, then, displacement and fixation of the uterus, hardness of the vaginal roof, and the occasional existence of a tumor in such close proximity to the uterus as to appear to be a part of it, as the only physical signs of peritonitis.

Let us now consider those of cellulitis, of subacute or chronic character.

In examining such a case, the most prominent abnormality, and that which is most likely first to arrest attention, is the existence of a tumor usually on the side of the uterus, and in about the situation of the broad ligament. This tumor in many cases feels to the touch not unlike the body of the uterus drawn to one side and fixed there; for it is often no larger in size, and not dissimilar in shape. Much less frequently the tumor may be felt posteriorly, and in some instances a tumor may be felt on both sides of the womb. These cases, however, are quite exceptional. The tumor is nearly always immovable, and can almost invariably be felt through the abdominal walls when firm pressure is made. The mobility of the uterus is generally diminished, but there is no such fixation as occurs in peritonitis unless that disease complicates the cellulitis. In a certain proportion of cases there is no impairment whatever of its mobility. There is usually no induration of the pelvic roof, unless in the immediate vicinity of the exudation, and commonly not even there. As a rule, the uterus suffers little or no displacement, and when displaced is not fixed in its abnormal position.

These are all the physical signs of cellulitis. Let us contrast them with those of peritonitis.

In peritonitis we have displacement and fixation of the uterus, diffused hardness of the pelvic roof, and commonly no tumor. In cellulitis we have always a tumor, generally on one side of the uterus, little or no hardness of the pelvic roof, and not much displacement or fixation. There would appear to be sufficient dissimilarity here on which to base a differential diagnosis, and in typical cases, I think, such a diagnosis can always be made, but unfortunately all cases are not

typical. Peritonitis is a very frequent complication of cellulitis, and occasionally cellulitis is sufficiently diffused to resemble very closely peritonitis.

The question might here be asked, What evidence is there that the physical signs just given are distinctive of these affections, even in typical cases, and are not common to both?

To such a question I would answer, that the evidence is of a twofold character, direct and inferential: 1. In the comparatively small number of autopsies that have been made on subjects dying while suffering from these affections, and in which any attention has been given to the question of differential diagnosis, the lesions discovered after death are such as correspond with the signs that I have mentioned. For example, the most carefully-observed cases of this kind, that I have been able to find, are those of Bernutz, which were all cases of peritonitis, and were published by him for the express purpose of showing how constantly this affection has been mistaken for cellulitis, and how very grievously Nonat had erred in this respect; consequently they were cases in which the question of differential diagnosis received the most careful attention. Now, the two most prominent physical signs of peritonitis that I have mentioned are, displacement and fixation of the uterus, and these were found in every one of Bernutz's autopsies. He says but little of hardness of the pelvic roof, and in his cases the presence of a tumor is the rule, and not, as I have stated it, the exception.

But, as all of these cases were of the acute form, the absence of hardness was to be expected; and, as they were of that very exceptional variety of peritonitis in which suppuration takes place and forms an encysted tumor, they constitute just that class of rare cases that I referred to as sometimes giving a tumor. In fact, Bernutz himself calls attention to the great rarity of suppuration in pelvic peritonitis. Now, the second kind of evidence is in reality nothing more than a corollary of the first (which has just been stated); for, if, in examining any given case, a tumor be found in the vicinity of the uterus, without any displacement or fixation of that organ, then

that tumor is not due to peritonitis. Moreover, both clinical and necropsic observations have shown that the adhesions of peritonitis are very persistent unless removed by pregnancy, while the tumors due to cellulitis are more commonly of only temporary duration (by which I mean a few weeks').

Let us now see how far the symptoms may aid us, when the physical signs leave us in doubt. I have already stated that these are seldom sufficient of themselves, but they are occasionally valuable aids. I would remark, then, in the first place, that in a very considerable number of subacute and chronic cases, perhaps in one-half, the symptoms are so slight and have so little reference to any inflammatory condition that they do not point to these affections at all, and afford us no assistance whatever. It is a very common circumstance to be consulted by patients for dysmenorrhœa, or irritability of the bladder, or suppression of the menses, or metrorrhagia, and find on examination evidences of extensive cellulitis, or peritonitis, of which, in most cases, no history can be given by the patients. They cannot remember having suffered from any illness to which it could be ascribed. In other cases, however, the history and symptoms are very marked. The one symptom which is most prominent in both affections is, pain in or about the pelvis. This pain is sometimes referred to one or the other iliac fossa, sometimes to the supra-pubic region, and sometimes to the back. Occasionally it shoots down the thigh, and may even cause retraction of the limb. There is nothing in this symptom on which we can predicate with any certainty which affection we are dealing with. It may be stated, however, that pain confined to one side, or shooting down one thigh, more frequently attends cellulitis, while pain of a paroxsymal character is oftener peritoneal. Retraction of the thigh is almost always the result of cellulitis. In the functional disturbances of the ovaries, uterus, bladder, and rectum, there is nothing that is distinctive of either affection. Suppuration, when it exists, is in most cases due to cellulitis.

I say when it exists, for in my experience it is of far less

frequent occurrence than we would be led to suppose from the statements of most authors on this point. For example, West says: "The tendency to suppuration and to the discharge of pus seems to be very great, since it occurred in twenty-seven out of fifty-two instances. This mode of termination of the inflammation appears also to be as frequent in cases independent of previous delivery or miscarriage as in those which are due to puerperal causes, since it happened in nine out of fifteen instances of the former kind, as well as in eighteen out of thirty-seven of the latter." McClintock says: "As regards the frequency of abscess in puerperal cellulitis, my clinical records show the following results: Of seventy cases, thirty-seven ended in suppuration with discharge of pus, and twenty-four of these burst or were opened externally." Of the comparative frequency in non-puerperal subjects, he gives no estimate, although one of his two chapters is devoted exclusively to this class of cases. Simpson estimates that suppuration occurs in about one-half of all the cases. Bennet, Grisolle, and others, also regard suppuration as the more frequent termination. Duncan gives it as his opinion that suppuration in these cases is even more frequent than the statements of any of these authors quoted would lead an inexperienced reader to suppose. Dr. Thomas, on the contrary, says: "Cellulitis proper, that is, uncomplicated by other diseases, rarely passes into a chronic state, but usually in the course of two or three weeks passes off by resolution, or ends in suppuration, the former being the more frequent termination." Yet in his tabular synopsis he gives a tendency to suppuration as one of the distinctive characteristics of cellulitis. Nonat states that the termination by suppuration, which has been regarded as very common by most physicians, is in reality very rare when the cellulitis has been subjected to proper treatment. Gallard found it only four times in fifty-three cases. Aran's experience agrees with that of Gallard, and Courty is of the same opinion as Aran. In my own experience, it occurred three times in forty-nine cases of cellulitis, and once in sixteen cases of peritonitis.

I have dwelt somewhat on this question of suppuration because I consider it an important one, and because this discrepancy in opinion and statement seems to me to be very significant. This is a question not of theory, but of fact; how, then, are we to explain such contradictory statements on the part of capable and conscientious observers? I think the solution is to be found in the circumstance, to which allusion has already been made, that a large proportion of cases are attended with so few or such slight symptoms, at the outset, that they escape notice, and are only detected when some subsequent ailment or functional disturbance causes investigation.

While, on the contrary, every case of sufficient severity to result in suppuration must almost inevitably challenge attention, nearly every author to whom I have referred bases his statistics or views on cases observed in hospital practice. Among puerperal patients, all suppurative cases would almost certainly be reckoned, while the milder cases might easily escape detection, and the same result would obtain in non-puerperal patients.

Among out-door patients complaining of uterine disorders, however, the case would be different. They almost always seek advice on account some functional derangement or local pain that necessitates a careful examination, and, among these, evidences of cellulitis or peritonitis could scarcely escape observation, and, indeed, are commonly found where the history given by the patient would never lead us to suspect their existence. I am aware of a possible source of error here, in the fact that suppuration may occur, and the pus be discharged through the rectum or bladder, without the patient's knowledge. But, even making allowance for this source of error, against which I have carefully sought to guard, suppuration is still a quite exceptional occurrence. I believe I have now mentioned all the symptoms that are of any value, and I think it is sufficiently obvious that they are insufficient of themselves for a diagnosis. Leaving now for a moment the consideration of subacute and chronic cases, let us see if we can do any better, or as well, in acute cases. Speaking from

a limited experience, extending to only ten recorded cases of cellulitis and six of peritonitis, I should answer, not so well, at least in the early stages, by which I mean the first week of the disease. In cases of this kind the symptoms become rather more valuable, and the physical signs rather less so. With the symptoms of peritonitis we are all sufficiently familiar, and pelvic peritonitis differs from general peritonitis only in being less severe and more localized; that is to say, we have the same pain, fever, nausea, and vomiting, dorsal decubitus, anxiety of countenance, etc., but the tenderness and pain are chiefly in the hypogastrium and pelvis, the tympanitis is entirely absent, or comparatively slight, and there is more commonly functional disturbance of the pelvic viscera. Now, in cellulitis we have also pain and tenderness in the hypogastrium, fever sometimes, but less commonly, gastric disturbance, and interference with the functions of the bladder, uterus, and rectum.

There is certainly no great difference here, and yet in a considerable number of cases there *is* such a difference in the physiognomy of the two affections as would enable an experienced observer to form a pretty correct judgment from the symptoms alone. The physical signs are somewhat better marked, but are by no means so distinct as in subacute and chronic cases. In peritonitis, physical examinations usually reveal nothing during the first five or six days but tenderness when the uterus or pelvic roof is touched, and pain on hypogastric pressure. After this, there begins to be a general hardness of the pelvic roof, with fixation of the uterus, and, if there be any distinct tumor, it will be found preferably in the posterior *cul-de-sac.* In cellulitis, on the other hand, besides the pain and tenderness when the uterus and pelvic roof are touched, there is also very early a feeling of fullness, which is commonly more pronounced on one or the other side of the uterus—not a well-defined tumefaction, such as occurs later, but an œdematous sort of swelling, which may resolve into a distinct tumor in two or three days, or after the lapse of a week or so.

These differences, slight as they are, are the only ones, I think, that clinical experience has thus far enabled us to point out. Another question might here be raised, and in fact *has* been raised, viz., whether there is any practical advantage to be derived from the differentialities of these affections, even assuming that this can generally be accomplished. I think it can be shown that there *are* palpable advantages, but I shall not now rehearse them, for in my judgment a question of this kind should never be entertained for a moment. It is the duty of those engaged in scientific investigations to take note of every fact or supposed fact that comes under observation, and to give the results of their inductions, without any reference to the question of immediate practical utility. It is only about a century ago that measles and scarlet fever began to be recognized as distinct affections, and we have only to go back as far as Sydenham to find the same distinction beginning to be made between measles and small-pox. Is it probable that there was any immediate practical utility apparent in the first separation of these affections from each other? The possible utility of pathological distinctions of this character is a matter which the future only can determine, and it is unphilosophical and idle to attempt to estimate it in advance. I now propose to speak very briefly of the frequency of these affections, of their causes, their diagnosis from other affections, and their treatment.

Of their frequency, only a general statement can be made. Courty, grouping both diseases together, considers that either as primary affections or complications of other maladies, they constitute about one-third of all uterine ailments. Aran found evidences of pelvic peritonitis in twenty-nine out of fifty-three autopsies of females dying in his service. But, as Mathews Duncan justly observes, these were practically selected cases. They were not fifty-three women taken at random, but fifty-three women taken from a gynæcological service, and suffering from ailments so severe as to cause death. Simpson speaks of cellulitis as a disease which "is by no means unfrequent in practice;" while West merely alludes to it as being of com-

mon occurrence. From my own experience, I should say that, while I believe these affections to be far more common than is generally supposed, I think they are by no means so frequent as Courty estimates. Of more than four thousand recorded cases of uterine disease of all kinds that I have seen during the past seven years in the different institutions to which I have alluded, I should estimate that not one in ten gave any indication of cellulitis or peritonitis, either past or present (this, of course, is only an approximate result, and refers almost entirely to out-door patients). The causes of these affections I shall merely enumerate, dwelling for a moment on one only: parturition or abortion, endometritis or ovaritis, operations or direct injuries, gonorrhœa, or exposure to cold, escape of fluids into the peritonæum, etc. Without stopping to discuss the efficiency or relative importance of any of the causes above enumerated, I will merely allude, in passing, to the theory of Mathews Duncan. Grouping together all the inflammatory affections, both of peritonæum and cellular tissue, now under discussion, he says: "The theory on which I insist is, that these inflammations are all secondary, that they are produced by inflammation of the uterus, or of the tubes, or of the ovaries, or by noxious discharges through or from the tubes and the ovaries, or by mechanical injury . . . . Of all the prolific causes, inflammation of the mucous membrane of the womb is, in my opinion, the most common, and this both in the puerperal and non-puerperal states."

Again he states, "Now, I assert that, in the majority of cases, pelvic abscess and metritis are stages and conditions of one disease." This theory I am inclined to regard as altogether too sweeping; and I see, moreover, no reason why the starting-point of the inflammation should be the uterus any more than the ovary or the peritonæum itself. I believe that local peritonitis may be as much an idiopathic affection as local pleurisy, and I am sure I have seen many cases of cellulitis and peritonitis that could not be traced to any concomitant or antecedent endometritis. This objection, however, may lose much of its force when I add that I do not look

upon the majority of cases of uterine catarrh that come under observation as being caused by inflammatory exudation, but regard them rather as instances merely of higher secretion. There is, too, a certain pathological law bearing on this point which should be kept in view. I can do no better than give it in the words of the distinguished President of this Academy, as published by him some fifteen years ago. Speaking of the varieties of epithelium and their distribution, he says: "Disease respects the distinctions made in regard to the different varieties of epithelium. In croup, the nasal passages are almost invariably affected, and the disease follows the course of the ciliated epithelium over the posterior surface of the velum and thence into the larynx and trachea, and not along the œsophagus into the stomach. Again, ... the uterine glands may be diseased for an indefinite period without the disease extending either to the uterine cavity or to the vagina; their canoidal epithelium being bounded both above and below by the scaly variety." Although this is a point of great interest, the limits of this paper will not allow me to pursue it further. With regard to the influence of the puerperal condition in giving rise to these affections, I find one statement in the work of Dr. Thomas which is not sustained by any of my own experience. He lays it down as a proposition, that "peri-uterine cellulitis is very rare in the non-pregnant woman, while pelvic peritonitis is exceedingly common." I think both members of this proposition need confirmation, and especially the first. Having pointed out some of the means by which these affections are to be distinguished from each other, we have now to consider with what other diseases they are likely to be confounded, and how a differential diagnosis may be made; and I shall here have to be very brief, for, although this is a most important part of the subject, it is too extensive to be treated here at any length.

The principal diseases from which they are to be differentiated are: hæmatocele, fibrous tumors, uterine deviations, displacement and fixation of the ovary, and fecal infarction.

Hæmatocele differs from both cellulitis and peritonitis in

being much more rare than either, in occurring with suddenness, and most frequently during a menstrual period, in being accompanied with hæmorrhage, in giving the constitutional symptoms of loss of blood, and on occasions a tumor, that is at first soft and elastic, and only becomes hard when peritonitis complicates it, or the fluid portions of the blood have been absorbed.

The most efficient of all means of diagnosis, in the majority of cases, however, is the use of the hypodermic syringe.

Fibrous tumors are distinguished by their mobility, the absence of pain or tenderness on pressure, by their evident attachment to the uterus, and by the absence of any signs of inflammation. The free mobility of a prolapsed ovary, whether enlarged or not, would serve to distinguish it from the tumor of cellulitis, with which alone it would be likely to be confounded. If the ovary be fixed in its abnormal position, such fixation would be an evidence of peritonitis. Uterine displacements are best determined by the uterine probe or sound. Fecal impaction, high up in the rectum, is likely to prove a source of error only so long as it is unsuspected. Nevertheless, I have known several errors of diagnosis on this account, and the real disorder aggravated by the use of opiates, given to relieve the symptoms of a cellulitis that had no existence.

On the subject of prognosis I must be still more brief. So far as danger to life is concerned, there is in most cases nothing to be apprehended. Functional derangement of the ovaries, uterus, bladder, and rectum, persists for a variable period, which is generally much less in cellulitis than in peritonitis. All evidences of cellulitis may pass away in a few weeks, but the signs of peritonitis, unless removed by pregnancy, generally remain for years.

The subject of treatment I shall also dismiss in a very few words, and these few will refer chiefly to subacute and chronic cases of the non-suppurating variety. I will merely observe that the acute stage of either of these affections should be treated on general principles. Anodynes, local depletion, fermentation, and sedatives, are the principal therapeutic agents.

After this stage has passed, the most efficient means of causing the absorption of the products of inflammation are, for both affections, successive fly-blisters over the hypogastrium, and the copious use of the warm vaginal douche every night and morning. The best apparatus for this is a keg that will hold two or three gallons, to the stop-cock of which a long India-rubber tube is attached. This keg, being filled with water, as warm as the patient can bear, is placed upon a shelf ten or twelve feet high, and the free end of the tube introduced as far as possible into the vagina, and the water allowed to flow in a constant stream against it back of the arteries. The rapid improvement that follows the use of these means, in the majority of cases, is astonishing even to those who have had much experience in the use of them.

Having now considered, as fully as the time of the Academy will permit, all the points to which I propose calling attention in this paper, it may be proper to give the statistics of the cases which have come under my observation. The whole number of cases of which I have kept a record is eighty-seven. Of these, twenty-two are so incomplete as to be of very little value, and I shall cast them out. The remaining sixty-five give the following results:

	Cellulitis.	Peritonitis.
Whole number of cases	49	16
Acute cases	9	7
In married women	44	13
In single women	5	3
In multiparous women	10	6
Tumor on right side	17	1
Tumor on left side	28	1
Tumor on both sides	4	14
Following labor or abortion	11	5
Following operations	5	4
Cause not ascertained	33	7
Resulting in suppuration	3	1

In conclusion, Mr. President, allow me to observe that no one can be more fully aware than myself of the very imperfect manner in which this subject has been presented; but, if

the remarks now made shall serve to awaken sufficient interest in these affections to induce others of larger experience and more mature views to contribute their knowledge to the settlement of the many vexed questions that have aroused them, my object will have been fully obtained.

Dr. T. G. Thomas, being called upon by the President, made the following remarks:

Mr. President, as I understand the paper of the evening, its author intends to deal with prognosis, treatment, and even pathology, very superficially; indeed, he has brought them in merely to give completeness to his excellent essay. The pith of the paper unquestionably consists in the following questions asked, and as I think ably answered by the writer: 1. Are there two distinct diseases which affect the pelvis of the female, one consisting in phlegmonous inflammation of the areolar tissue of this part, and the other in inflammation of the peritonæum covering the pelvic viscera? and, 2. Can these diseases be distinguished from each other during life? These are the points which the essayist wishes to place *sub judice;* these the questions upon which he wishes to bring the experience of the Academy to bear.

For a number of years my attention has been especially directed to these two points, and I feel that it is incumbent upon me to speak of them. Early in my experience in gynæcology, I noticed that there were two entirely different varieties of peri-uterine inflammation which presented themselves at the bedside. In the one, there was fixation of the uterus, hardness of the whole pelvic roof, tenderness over the whole hypogastrium, but no hard and tender mass at any point. In the other, there was fixation of the uterus only on one side; in one iliac fossa, conjoined manipulation distinctly revealed a hard, round, tender mass, not movable, but attached to the pelvic walls, and feeling much, in some cases, like a fibrous tumor. In the opposite iliac fossa no hardness, tenderness, or other pathological condition, would be found to exist. For a long time I was puzzled by this singular difference in cases, always classified as one and the same affection. In time, I was

enabled to examine, *post mortem*, one of the first class, and I found no evidences of inflammation of the areolar tissue at all, but abundant evidences of peritonitis, affecting the pelvic peritonæum. Since that time, I have repeated this pathological research on several occasions, and usually with the same result. I say usually, for the reason that these two affections very often complicate each other; as often, perhaps, in slight degree, as pneumonia and pleurisy are found to exist together.

In the second class of cases, *post-mortem* evidence had not to be waited for. In this I was repeatedly called upon to open large abscesses due unquestionably to cellular inflammation.

In time, my mind became made up to the fact that one of the classes of cases was pelvic peritonitis, and the other pelvic, or, as I prefer to call it, peri-uterine cellulitis, and as time passed on, and my experience in diagnosis increased, I learned to rely upon my ability to differentiate these affections at the bedside.

To both of Dr. Brown's questions then, sir, I would answer in the affirmative. There exist two inflammatory affections of the peri-uterine pelvic tissues, one consisting in a true peritonitis, the other in a true cellulitis. I believe, furthermore, that these affections may be differentiated at the bedside, by one whose diagnostic powers have been rendered acute by lengthy and faithful study directed to this point.

It has been said here to-night that the two affections often, nay usually, complicate each other. I admit this, but what of it? Only this, that hence the greater reason for faithfully striving for the means of differentiating them. Pneumonia and pleurisy, pericarditis and endocarditis, and other diseases, often complicate each other. Is this a sufficient reason for our confounding them? Are the interests of pathology to be subserved by our supinely accepting that for impossible which proves difficult? No, sir; the fact of their often complicating each other, even the more important fact of their often being so completely blended that differentiation is impossible, only points to the necessity for greater skill on the part of the diagnostician. It is an old French adage, "Plus

on s'élève, plus l'horizon s'avance." It is true in diagnosis as in every thing else.

Some years ago, after reading the views of Prof. Austin Flint upon the existence of a direct mitral murmur, I strove long and carefully to find it.—Repeated failures in the immense field of Bellevue Hospital made me arrive at the conclusion that no direct mitral murmur existed. One day as I was leaving the hospital-wards, I met Dr. Flint, and told him my conclusion. "Come with me into my wards," said he, "and I will convince you of the truth of what I have said." I went back, and came out perfectly satisfied that he was right, and never since that day, and this occurred nearly ten years ago, have I found any difficulty in detecting the direct mitral murmur. In a similar way have I convinced many who have attended my clinique, of the truth of the position which I have taken here to-night.

Besides, sir, making a diagnosis does not involve the necessity of determining every complication of the diagnosticated disease. I do not say that such determination is not desirable, I merely say that it is not essential. If a diagnosis of pneumonia, as the main disorder, be made, it is not invalidated by *post-mortem* evidence of pleurisy which was not recognized during life. One was the great, the leading phenomenon; the other was the unimportant epiphenomenon. So the diagnostician will generally determine as to the presence of peritonitis, even if he do not recognize a coexisting cellulitis, and *vice versa*.

The thorough utilitarian may ask, Is any real immediate practical good to be obtained from such a differentiation? I would, like the essayist, refuse to consider that question. Knowledge always gives power, knowledge is always of value; knowledge is ever to be preferred to ignorance, as surely as light is preferable to darkness.

The hour is now so late that I fear to trespass longer upon the time of the Academy. Before I close, sir, allow me to indulge in a prophecy. The day is not far distant when no writer on gynæcology, who has a proper regard for his reputa-

tion for care and skill as an observer, will venture to leave out of consideration the essential differences which exist between these two diseases. The time is almost at hand when they will be recognized at the bedside and in the lecture-room as being as distinct one from the other as are pneumonia and pleurisy.

The Academy then adjourned.

STATED MEETING, APRIL 4, 1872. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DR. HENRY GRISWOLD was elected a Resident Fellow.

The Secretary announced the death of Prof. SAMUEL H. DICKSON, M. D., of Philadelphia, on March 31, 1872, aged seventy-four years, a Corresponding Fellow of the Academy.

Dr. ISAAC E. TAYLOR then read the paper of the evening, on the "Mechanism of Spontaneous Active Uterine Inversion; and the Reduction of a Case of Complete Inversion by the Combined Rectal Ana-Vaginal Taxis." (Published elsewhere.)

The Academy then adjourned.

STATED MEETING, APRIL 18, 1872. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

THE PRESIDENT announced the receipt of the *Edinburgh Medical Journal* for April, 1872.

The PRESIDENT exhibited an ovarian tumor removed from a lady a few hours before the meeting. The weight of the whole tumor was fifty-five pounds.

Dr. J. PARIGOT (by invitation) then read a paper entitled "The Influence of Philosophical Speculations on the Practice of Medicine, and Medico-Legal Questions of Insanity."

The Academy then adjourned.

STATED MEETING, MAY 2, 1872. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DR. STEPHEN W. ROOF was elected a Resident Fellow.

Dr. E. H. JANES exhibited a sample of *styptic cotton*, and described the method of preparing it.

Dr. MOREAU MORRIS then read the paper for the evening on "Cerebro-Spinal Meningitis." (Published elsewhere.)

The Academy then adjourned.

STATED MEETING, MAY 16, 1872. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

THE PRESIDENT announced the receipt of Annual Report of the Trustees of the Museum of Comparative Zoology at Cambridge for 1871, and the *Edinburgh Medical Journal* for May, 1872.

Dr. A. C. POST stated that he had recently, in the Pennsylvania Hospital, witnessed an amputation at the hip-joint by Dr. ADDONELL HEWSON, who applied a tourniquet to the abdominal aorta in such a manner that there was no loss of blood. The vessels were secured by torsion.

The PRESIDENT read extracts from a letter from Dr. AXFORD, of Flint, Mich., giving an account of two cases of hydrophobia successfully treated by opium and large doses of castoreum.

The PRESIDENT exhibited a fibroid tumor involving the whole mammary gland, removed that afternoon.

Dr. G. M. BEARD then read a paper entitled "Recent Researches in Electro-Therapeutics." (Published elsewhere.)

The Academy then adjourned.

STATED MEETING, JUNE 6, 1872. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DR. EDWARD JOHN TILT, of London, Eng., was elected a Corresponding Fellow.

The PRESIDENT announced the receipt of Daily Bulletin

and Weather Maps for May 29, 1872, from General Meyer, Chief Signal-Officer, U. S. A.; also an obituary notice of the late Charles A. Lee, M. D., from Dr. J. M. Toner, as contributions to the library.

Dr. A. C. Post reported a case of bronchitis, with impending suffocation, that was relieved in a few minutes by the application to the chest of twelve large tumblers as dry cups.

Dr. Roberts inquired if that was not a proper case for bleeding. Dr. Post, in reply, said that twenty-five years ago he probably would have bled the patient, but at present he does not so often resort to venesection as formerly.

The President remarked that in such a case the great object was to draw blood from the lungs for a time.

The President then announced the paper of the evening, entitled—

REPORT OF THREE CASES OF SYPHILIS TREATED BY SYPHILIZATION.

By JOSEPH C. HUTCHINSON, M. D., of Brooklyn, N. Y.

When Prof. Boeck, of Norway, visited this country, in 1869, I had under my charge at the Brooklyn City Hospital several stubborn cases of tertiary syphilis which he kindly consented to take charge of and treat by syphilization. I was impressed not only by his active zeal and earnestness, but also with the honesty of his motives and the sincerity and simplicity of his character, and did not hesitate, therefore, to place my patients entirely at his disposal. The cases are made public both in compliance with his request and on account of the intrinsic importance of the subject.

The progress of the treatment was carefully observed by myself, as well as by many other gentlemen who were interested in the question. The disease had resisted all the usual remedies, and, as the patients steadily grew worse, they promptly consented to have syphilization practised. Their coöperation was thorough and complete, and they soon manifested the greatest interest in having the inoculations regularly

performed. Not having seen the practical effects of syphilization, I was unwilling to have any patient syphilized who had a prospect of being relieved by the ordinary methods of treatment. The cases here recorded subjected the treatment by syphilization to the severest test, and, having been repeatedly mercurialized, they were among the class of cases which Prof. Boeck regards as the least amenable to this plan of treatment. Nevertheless he was anxious to submit syphilization to the most rigid ordeal.

Case I.—John S——, aged twenty-eight, native of Ireland, by occupation a coachman, entered the Brooklyn City Hospital September 23, 1869. His general health had been good before he contracted syphilis. His habits of life have been very irregular; he has been addicted to the use of alcoholic liquors. His experience in venereal disease has been large and various; states that he had a sore upon his penis at the age of sixteen, that he has had several others since that time, also two or three attacks of gonorrhœa. About three years ago he had a sore on penis, which was soon followed by alopecia. Took no medicine at this time. At the end of a year or fourteen months, having contracted no other sore meanwhile, he had a scaly eruption on his face; this remained about two months, and then seemed to yield to local applications. About five months since a circle of small sores appeared upon the glans penis shortly after an exposure. These were slow in healing, persisting over four months. Three months ago began to have sore-throat, which has constantly increased in severity until the present time. States that he had taken no medicine before entering hospital except a few (about six) "blue pills."

Examination, on admission, shows the uvula, tonsils, and soft palate, to have been almost wholly destroyed by the ulcerative process; ulceration extends into the pharynx; larnyx probably involved, since there is almost complete aphonia. The ulcerations in fauces and pharynx have an ashen-colored base and discharge a thick muco-purulent matter. Swallowing is difficult, as well from the tenderness of the parts con-

cerned as from regurgitation through the nares. Small patches of eczema are observed on scalp, face, and neck. The inguinal, post-cervical, and epitrochlear glands, are enlarged and indurated; numerous cicatrices on penis and in groin. Emaciation has been rapid; has no appetite; is confined to the bed.

November 1*st.*—Treatment by means of mercurials and iodide of potassium has been perseveringly tried without benefit; has had cod-liver oil, ferruginous tonics, and a supporting regimen in such quantities as he has been able to appropriate. Ulceration has progressed, and it is now impossible to open the mouth wide enough to obtain a satisfactory view of the parts. The following prescription has been employed during the last two or three weeks:

℞. Hydrarg. chlor. corros.,	gr. j.
Potassii iodidi,	ʒ j.
Sol. ferri. pyrophos.,	f. ℥ ss.
Emuls. ol. morrhuæ,	f. ℥ vss.
M. Sig. One tablespoonful thrice daily.	

3*d.*—With the consent of the patient, pus from a chancroid is this day introduced in symmetrical positions on the chest by three inoculations on each side; all other treatment to be suspended, except local applications.

6*th.*—The inoculation only partially successful; decided pustulation not produced. Inoculated with matter from a typical inoculation on another patient.

26*th.*—Reinoculation has been practised on every third day when a pustule results; when it does not, matter is obtained from other sources and daily introduced as before. Patient says he feels better. The following was ordered:

℞. Spts. etheris comp.,	f. ʒ vj.
Tinct. gentian. do.,	f. ℥ v.
M. Sig. One teaspoonful thrice daily.	

December 4*th.*—Inoculations frequently fail to take effect, and never produce a large pustule. Received this day fresh virus from Charity Hospital, New York; as the chest appears to be no longer susceptible, introduced the virus into the arms.

It is customary with Dr. Boeck, when the inoculations habitually fail, to give small doses of the iodide of potassium for two weeks, and then recommence inoculations; the system is supposed thus to become more susceptible to the virus. Accordingly, ordered the following:

℞. Potassii iodidi, ℈ij.
Aquæ, fl ℥ iv.
M. Sig. One tablespoonful thrice daily.

Have made topical applications to fauces of strong solutions of the nitrate of silver and chromic acid. No marked change is observable.

18*th.*—Recent inoculations have been more successful. The condition of patient has improved; has better appetite and more strength. Throat has improved, but aphonia continues.

26*th.*—Medicine has been discontinued. Daily inoculations have been made upon chest or arms; they sometimes take, but not very vigorously; occasionally they wholly fail. Condition unchanged.

January 8, 1870.—Inoculated thighs with matter from Charity Hospital. Continue inoculations on chest and arms, but they seldom succeed.

12*th.*—Debility and pain having increased, the iodide of potassium was prescribed again. Daily inoculations continued; those on the thighs are more effective than those in other situations. The ulcerative process in throat has made no progress since the present treatment was instituted; aphonia continues; eczema has nearly disappeared; general condition somewhat improved.

February 1*st.*—Inoculations still practised upon arms and thighs; they do not uniformly succeed. Extreme emaciation and debility continue; has irritative fever, and a troublesome laryngeal cough; opiates are required at night.

20*th.*—Patient no longer susceptible to any virus that can be obtained. The iodide of potassium, to which the tincture of gentian has been added by Dr. Boeck's advice, is again administered.

25th.—For the first time in six weeks, has been strong enough to get out of bed and go about the ward; aspect has greatly improved; gains rapidly in flesh and strength; sleeps and eats well; does not sweat at night; cough much less troublesome. Throat has entirely healed.

March 12th.—Several inoculations have been tried, but without effect. Potassium discontinued on 7th inst.

20th.—Right thigh and knee painful and swollen; relieved by local applications. Ulceration in mouth and nares has again set in.

May 26th.—Has been unable to leave his bed on account of the pain in knee. No improvement in general condition.

August 3d.—Died from exhaustion.

Summary.—This case entered the hospital in a deplorable condition. He was rigidly subjected to the most orthodox treatment, medicinal and hygienic, without the slightest improvement; indeed, he steadily grew worse, until syphilization was begun. The ulcerations in the throat, which were progressing when this treatment was instituted, did not increase subsequently. A large proportion of the inoculations failed. Dr. Boeck says that, in those patients who have for a long time been treated by mercury, it is very difficult to obtain effective inoculations, and it is not rare that after a short time the result is negative. At the end of three and a half months the inoculations with virus, from whatever source obtained, cease to produce the slightest effect. Five days subsequently it is recorded that, "for the first time in six weeks, has been strong enough to get out of bed and to go about the ward; aspect greatly improved; gains rapidly in flesh and strength; sleeps and eats well; does not sweat at night; cough much less troublesome; throat has entirely healed." This result confirms the statement of Prof. Boeck, that as a rule the syphilitic symptoms disappear when the virus ceases to produce appreciable effects. Before the end of another month the grave symptoms returned, prostration increased, ulceration in the throat and nares again set in, producing difficulty in swallowing and regurgitation of fluids through the nares, and he died from exhaustion August 3d.

The good effect of the syphilization in this case was for a time very marked, and, I must confess, was surprising to me; and I do not think that Prof. Boeck himself expected so great an amelioration of the symptoms as took place. It seems to me to be a legitimate conclusion that this patient's life was, at least, prolonged by this method of treatment.

Case II.—James C., aged twenty-one; sailor. Admitted August 16, 1869. Patient states that his health has been good. Five months ago, ten days after an exposure, a small sore appeared near the meatus urinarius, followed soon after by other sores upon the glans. These healed speedily, to be followed by another, in April (without fresh exposure); this also healed rapidly. Early in May, began to have sore-throat, and cervical glands became enlarged. June 8th, an eruption made its appearance on the forehead, and since that time it has spread to the face, trunk, and limbs.

Examination, on admission, discovers patches of ulceration in fauces and pharynx; the epitrochlear and inguinal glands are enlarged and indurated. A very extensive rupial eruption in various parts of the body. Is emaciated and very feeble. Treatment by means of mercurials (internally and by fumigation), iodide of potassium, sarsaparilla, etc., was instituted.

September 2d.—No improvement having taken place under the usual treatment, syphilization was, with the consent of the patient, inaugurated.

November 30th.—Inoculations with matter from a soft chancre on chest have been systematically practised every third day, and are invariably successful.

December 14th.—No further effect produced by inoculation on chest, by matter from whatever source. Large crusts form on the sites of inoculations; they increase in size and become confluent. Rupial crusts on face have diminished in size. General condition somewhat improved.

February 1st.—Reinoculations have been practised successfully on arms and thighs, which have become covered with extensive crusts, while the chest is comparatively free from them. Large ulcers continue in the throat, and there is a constant

fetid discharge from the nose. Although the patient appears to be in better physical condition, is yet too feeble to leave his bed.

21*st.*—Inoculations are now seldom effectual; they are accordingly discontinued, and the iodide of potassium, with a vegetable bitter, is prescribed.

March 8*th.*—Recommenced inoculations, but patient is no longer susceptible to virus taken from any source.

28*th.*—General physical condition has improved; the patient leaves his bed for the first time in five months. Eats and sleeps well. The ulcerations in throat and pharynx are healed.

May 3*d.*—The crusts following inoculations have nearly all disappeared. Patient weighs one hundred and eighty pounds —more than at any previous period. Says he is "a well man."

July 9, 1870.—Has continued to improve in health and strength, and is this day discharged cured.

Summary.—This patient was not so feeble as the first, although he was sufficiently so to keep his bed for five months after he entered the hospital. He was subjected to the mercurial treatment for two months before he came into the hospital, and for two weeks afterward, without benefit. Between five and six months after the inoculations were commenced, there was a decided improvement in his symptoms. The inoculations were quite effective. At the end of six months he became susceptible, and, coincidently with this, all his symptoms improved. Nine months after syphilization was instituted, he was discharged cured.

Case III.—John Sullivan, age thirty-seven; by occupation a sailor. Was admitted to the Brooklyn City Hospital August 1, 1869. Has had several venereal sores, one of which, contracted about two years ago, was probably a hard chancre. His eyebrows fell out, but not the hair. No eruption or persistent sore-throat. In September, 1867, he entered the hospital at Savannah, to be treated for severe nocturnal pains in the shin-bones; and while there had an attack of malarial fever. Was there treated by means of quinia, arsenic, and mercurials.

Pains have continued until the date of admission, at which time he complains of severe pains in his joints, of a somewhat fugitive character, and in his shin-bones, aggravated at night; has severe frontal headache. Nodes on both tibiæ, which are tender to the touch. Many superficial lymphatic glands are enlarged and indurated. Is emaciated, feeble, and anæmic.

November 8th.—The usual treatment (mercurials, iodide of potassium, iron, bark, cod-liver oil, etc.), without benefit; it was abandoned, and syphilization was substituted by Prof. Boeck. Matter was taken from a well-marked pustule on a patient inoculated with virus from a soft chancre. Has been confined to his bed during the past week.

20th.—Reinoculations have been made every third day; distinct pustulation results from four out of every six trials, but the pustules are not large. Pus used to-day was taken direct from a fresh chancroid.

December 2d.—The inoculations are not very effective; accordingly, the following was prescribed by Dr. Boeck:

R.	Potassii iodidi,	ʒj.
	Spts. etheris comp.,	fl ʒjss.
	Tinct. gentian. do.,	fl ʒjss.
	Aquæ,	q. s. ad fl ℥vj.
	M. Sig. One tablespoonful thrice daily.	

11th.—Has been reinoculated on chest every day since last entry. To-day begin on the arms. The pustules are more complete than formerly. The nocturnal frontal and other osteoscopic pains are not felt every night, and when present are less severe than formerly.

21st.—Has been inoculated from pustules on his own body since November 23; on an average, seven out of twelve trials have been successful. The potassium mixture has been discontinued. The patient has perceptibly improved, and is now able to get up and walk about the ward; and on pleasant days he goes out-of-doors.

27th.—Began to-day to inoculate the thighs.

January 25th.—Inoculations do not take well; the iodide

of potassium is again ordered. Has had a recurrence of nocturnal pains, but they are not so marked as before.

February 11, 1870.—Dr. Boeck remarks: "The benefit from the treatment in this case, I believe, is very satisfactory; the pains in the bones are gone, the hyperæsthesia of the skin is removed, the tibiæ are no longer sensitive, his health is improved, his appetite is excellent, and he has gained flesh."

April 1*st.*—Discharged cured.

November 3, 1870.—Readmitted to hospital on account of diarrhœa. Complained at this time of none of the symptoms of syphilis.

Summary.—This patient had been treated for more than two years with mercurials, for three months in the Brooklyn City Hospital, without material benefit. The inoculations took poorly. Some improvement in his symptoms was noticed at the end of a month, and in four and a half months the inoculations ceased to be effective. He was discharged free from all syphilitic symptoms. Seven months subsequently he was readmitted to the hospital on account of diarrhœa, and presented no traces of syphilis.

The three cases here reported went from bad to worse under the various treatments which had been adopted, and were regarded as utterly hopeless when Prof. Boeck began the inoculations. Dr. M. H. Henry (editor of the *American Journal of Syphilography and Dermatology*), who visited the patients with Prof. Boeck, declared that they were the most unpromising cases he ever beheld. The experience afforded by three cases of tertiary syphilis treated by inoculation is insufficient to justify us in determining the value of syphilization as a remedy for syphilis. A much larger experience would be necessary for this purpose. It is sufficient, however, to induce me to question the justness of the inexorable rule laid down by Messrs. Lane and Gascoyne of the London Lock Hospital, and indorsed by one of our highest American authorities, Prof. Bumstead, who observed the effects of syphilization in three cases at the New York Charity Hospital,* "that syphili-

* See *American Jonrnal of the Medical Sciences.*

zation is not a treatment which can be recommended for adoption." In such chronic cases as are here reported, which fail to yield to more ordinary treatment, syphilization is surely a treatment which *can* be recommended, and I feel that I would have been justly censurable had I omitted to avail myself of the advice of so experienced and conscientious an observer as Prof. Boeck has proved himself to be both here and abroad.

It cannot be expected, from the limited experience herein detailed, that I shall indorse all the positions of the advocates of syphilization, but I feel that I shall be warranted in proffering this plan of treatment, novel and unpopular though it be, to such patients as shall persistently defy and grow worse under the plans that are now authorized and accepted; and, that such cases do occur, I presume no student of syphilis will deny.

Dr. A. C. Post said he thought the theory of syphilization, or the introduction of syphilitic poison into the patient until he is no longer susceptible to it, a false one. If the theory be correct, the chancroidal inoculation would be the proper one. It is supposed by some, that the establishment of a large number of pustules, by whatever means, in different parts of the body, tends to facilitate the elimination of the poison from the body, and to cure the disease.

Dr. I. E. Taylor said that syphilization had been used in Charity Hospital before Dr. Boeck came to this country, and after his arrival there were eighty or ninety patients on whom syphilization was tried, but without success; some of the cases were afterward cured with mercurials.

Syphilization was adopted in the case of a woman in Bellevue Hospital, but without success; mercurials were afterward used successfully. At the time Dr. Boeck left this country, no good results had followed syphilization in Charity Hospital, and I must confess that I was not favorably impressed with it, and have not resorted to it.

Dr. Keyes said the fact that relief did occur in these cases, so well reported by Dr. Hutchinson, after all other methods of treatment had failed, warrants the use of such

treatment in similar cases. In most cases the early eruptions of syphilis get well under almost any treatment, while others go on getting worse. He referred to a case where the bones of the nose had become affected, the shins were swollen, painful, etc., purpura had been developed by seven-grain doses potass. iodid. A change to the country, cod-liver oil, and smaller doses potass. iodid. produced a great change in their course in a few weeks. He thinks these cases improve much more rapidly in the country than in the hospitals here. He believes that the first infecting chancre protects nearly all. The chancre may or may not indurate. He related a case where secondary symptoms followed a soft chancre. Infecting and non-infecting would be better terms. It is better not to commence constitutional treatment in every case, but, when in doubt, wait a few months for an eruption.

Dr. Roberts said that to him there appeared to be some parallelism between syphilization, as practised by Dr. Boeck, and the practices of vaccination and inoculation. Each purports to introduce into the system the specific virus of the disease, to prevent any subsequent or further infection of the system. As a general rule, this is true of specific diseases, but not of all; and not so of syphilis, which frequently recurs in a primary form in those previously infected. Vaccinia is supposed to introduce into the system the small-pox of the cow, and inoculation does introduce the small-pox of the human; and persons so infected are very generally, but not always, protected against a second attack. In order to do this, if it can be done at all, as to syphilis, it would be necessary to take primary syphilitic virus from an infecting source. If the soft chancre be not such, it is useless, as was done in Dr. Hutchinson's cases, to inoculate with it; but he (Dr. Roberts) was satisfied that he had seen soft chancres followed by infection. So, too, of taking virus from inoculated sores on the patient's own person. This would be equivalent to taking vaccine virus from a revaccination, which is not done, because it has then, by passing through the system once or oftener, lost some or much of its primary infecting power. One of Dr. Hutchinson's cases,

after seeming temporary improvement, died; the remedy, if such it be, having been applied too late. The other two recovered, after some months of uncertainty; but it should be borne in mind that these men had the hygienic advantages as to care, diet, regimen, of a well-regulated hospital, took a good many remedies of one kind or other, hydriodate of potassium, for instance; and, lastly, that the worst cases of secondary and tertiary syphilis not unfrequently get well by mere lapse of time, and the *vis medicatrix* of Nature. When syphilis was first discovered in the West India Isles, the natives treated themselves successfully without mercury or syphilization, by sarsaparilla, guaiacum, and mezereon, which even now sometimes suffice. Still, as there is a chance that repeated infection of the system may at length destroy its susceptibility to the poison, and the influence of the poison itself, and recovery thereby ensue, the practice does not seem irrational, and in bad cases might be tried with a hope of success. But it might also be a question whether, if it did not succeed, the patient would not be further endangered. Dr. Roberts was satisfied of having seen a case of secondary syphilis, such as primarily go on to tertiary, and persist in spite of remedies, simply because the woman would not discontinue her cohabitation with the still infected individual who had originally diseased her. Longer experience is required to show whether syphilization, after the manner of Dr. Boeck, is entitled to any, or how much, reliance as a cure for syphilis, or what will be its effect.

The Academy then adjourned.

Stated Meeting, June 20, 1872. Dr. E. R. Peaslee, President, in the Chair.

The President announced the reception of the *Edinburgh Medical Journal*, for June, 1872.

Dr. E. R. Squibb made some remarks on "The Current Materia Medica."

Dr. G. M. Smith then read the following paper:

LACTIC ACID AS A CAUSE OF DISEASE WHEN EMPLOYED AS A REMEDIAL AGENT.

By GOUVERNEUR M. SMITH, M. D.

Mr. President and Fellows of the Academy:

As relevant to the subject which Dr. Squibb has so ably presented to the Academy this evening, I will simply offer a few thoughts in relation to a principle which is interesting to the medical profession on account of the various aspects which it exhibits for our study. Reference is made to the principle known as *lactic acid.* The physiologist regards it as being one of the component parts of several of the normal secretions; the pathologist views it as the *materies morbi*, in a so-called diathesis; while the therapeutist receives it from the hands of the pharmacist to employ either *per se*, or in the form of a saline in the relief of disease. It is peculiar, in the fact that it is elaborated both in the animal and in the vegetable kingdoms, and can be readily artificially produced.

In the animal kingdom, lactic acid is found occurring as an ingredient of the gastric juice, in the proportion of 4.78 in 1,000 parts; associated with soda, potassa, and lime, it is detected in the urine in proportions varying from 1.50 to 2.60 in 1,000, while the same alkaline lactates appear in the sweat, in the ratio of about .317 in 1,000. Liebig and Robin affirm that the lactates are to be found, and are produced, in the muscular tissue. In the vegetable kingdom, Wittstein has observed lactic acid in the *Solanum dulcamara*, and in the fresh sap of the grape-vine. It is unnecessary to allude to the methods of artificially preparing this principle. To Schele has been attributed the honor of having first isolated this organic acid.

The dose of lactic acid, as directed in the U. S. Dispensatory, is from one to three grammes *per diem*, and as a remedy has been recommended by Magendie and Handfield Jones, in certain forms of dyspepsia, and in disorders accompanied by phosphatic deposits in the urine. The salts of lactic acid, as is well

known, vary in dose according to the base associated with the acid, and are prescribed at the present time in maladies of very diverse character.

In studying the therapeutical properties of medicinal substances, we are naturally led to inquire if the drugs in question possess any toxicological qualities. Now, it is with the view of determining whether or not the exhibition either of lactic acid or of its salts is ever attended with disagreeable *sequelæ*, that these thoughts are offered to this Academy, in the hope of eliciting the experience of its Fellows upon this subject.

It may not be inappropriate to state the reasons which have recently more particularly called my attention to this inquiry. An article appeared in the *British Medical Journal*, of December 23, 1871, from the pen of Balthazar W. Foster, M. D., Professor of Medicine in Queen's College, Birmingham, etc., entitled "The Synthesis of Acute Rheumatism." This paper, in the form of a reprint, was a short time ago sent to me by the author, and a few of the interesting statements therein contained are herewith presented as worthy of consideration.

Dr. Foster states that he "was engaged in completing an inquiry into the effects of different drugs on the sugar excretion in *diabetes*," and was led to observe the action of lactic acid. The first patient in whom he especially noted the behavior of the medicine was a man aged thirty-one years, who had suffered several months from melituria, and who, on admission to the General Hospital, was passing one hundred and eighty onces of urine *per diem*, containing forty-nine grains of sugar to the ounce. After treatment for two weeks on a restricted diet, the urine was diminished in quantity to one hundred and sixteen ounces *per diem*, containing but thirty-six grains of sugar per ounce. The patient was now placed under the influence of lactic acid, fifteen minims at a dose four times a day, the dose being doubled on the morning of the second day. Before the expiration of forty-eight hours the patient complained of acute pains in the joints, and the medicine was discontinued. As this symptom was regarded as a

mere coincidence, the patient on recovering from the arthritic difficulty was again placed under the influence of lactic acid. A repetition of the pains was the result, an increased general temperature, while a number of joints presented the appearance of typical specimens of rheumatic arthritis. The medicine was again stopped, and a subsidence of the inflammatory condition was again immediately noticed. During the use of the lactic acid there appears to be an improvement in the diabetic symptoms, and the patient was placed under a further trial of the drug. It soon became manifest that small doses of the remedy could be tolerated, but, if these were increased, rheumatic symptoms would be immediately developed.

While this patient was under treatment, another one presented himself at the hospital, likewise suffering with *diabetes*, who, on being placed under the influence of lactic acid, was on the fourth day attacked with symptoms of rheumatism. The acid mixture was discontinued, and in two days the patient was relieved of his arthritic complication.

Dr. Foster, in his concluding remarks, says: "In the first case, at least six well-marked arthritic attacks occurred; in the second case, under conditions less favorable for observation as to duration of treatment and place, one well-marked attack occurred. The phenomena corresponded in all respects to those which are characteristic of acute articular rheumatism. They came on when the acid was taken, and ceased when it was discontinued. When moderate quantities of the acid were tolerated, an increase in the dose was succeeded by the painful inflammation of the joints. Coinciding with the development of the articular affection, was the appearance of perspiration, at first only slight, but afterward, in the more severe attacks, copious and acid. These facts have dispelled the last lingering doubt in my mind as to the truth of the lactic acid theory of rheumatism. . . ." Dr. Foster further says: "In health, no doubt, much larger quantities of lactic acid than any given in my cases would be excreted without producing any perceptible disturbance in the bodily functions.

The acid would escape by the skin, the kidneys, or, after oxidation, as carbonic acid and water. It cannot be justly argued that the quantities of acid taken by my patients were too small not to have escaped in this way. The conditions under which the drug was given must be borne in mind. In diabetes, we have a state of suboxidation very unfavorable to the conversion by oxidation of new compounds; and, in Wright's case, this was aggravated by the serious pulmonary combinations. Associated with these, there was a dry and branny state of the skin, highly unfavorable to the elimination of the lactic acid by one of the common channels. Lastly, the well-known persistent acidity of the urine in diabetes points to a preëxisting hyperacidity of the fluids."

These observations of Dr. Foster are exceedingly interesting, as tending to show the correctness of the theory attributing arthritis to a lactic-acid diathesis, as so strongly contended by Fuller a few years since, and also as tending to exhibit that lactic acid, as artificially supplied to the system, may induce mischief. No one, who has examined the recent literature on the subject of rheumatism, can have failed to have had his faith somewhat shaken in the theory to which allusion has just been made. Clinical study of acute articular rheumatism, in dispensary, hospital, and private practice, has, however, led me to give a preference to the alkaline treatment, and to incline to the belief in an acid *materies morbi* in the disorder in question. While the different methods of treatment, which have of late been advocated for the relief of rheumatism, may be as efficient as that known as the alkaline method, nevertheless such success does not prove that the etiological views of Fuller, and of others of like mind, are incorrect.

For example, in the London Hospital Reports, we read of the treatment of rheumatic fever by free blistering, as recommended by Herbert Davies, in which either armlets, wristlets, thighlets, leglets, and fingerlets of blistering-plaster were applied " near to, but not upon every *joint inflamed*, at the very *height* of the inflammatory stage, when the local

pains were the most severe and the constitutional disturbance the *greatest*," or in which strong *acetum lyttæ* was painted in zones round the joints. Now, the chief design of such treatment was, to induce an elimination of a *materies morbi* from the blood through serous discharges, and was not with the intent of exciting a mere sanguineous derivation. Furthermore, Dr. Davies advocates a mild alkaline treatment, as a useful adjunct to the means just alluded to.

In Guy's Hospital Reports, G. Owen Rees reports cases of rheumatic fever successfully treated by lemon-juice. In such instances we can readily suppose that the alkalies naturally associated with the citric acid were as efficient in correcting an acid diathesis as are the alkalies when administered either in the form of Rochelle salt or when given in other saline forms as artificially prepared.

Again, in Guy's Hospital Reports, there are recorded a number of cases of acute rheumatism, occurring in the practice of Drs. Gull, Sutton, and Rees, which made favorable recoveries under a simple mint-water or an expectant treatment. We learn, from such facts, that patients, when placed under favorable hygienic conditions, will as readily recuperate from acute rheumatism as they will, as is well known, from many other disorders, and convalesce more rapidly than if they had been subjected to meddlesome medication.

These apparently diverse plans of treatment subserve to a certain extent a similar purpose, being, as has been said, either eliminative of or chemically antagonistic to some inimical principle. As there has been a pretty general belief that the *materies morbi* of rheumatism was *lactic acid*, we have in the interesting cases narrated by Dr. Foster such strong corroborative facts as to warrant us in confirming our faith in such supposition.

A question of practical importance here arises, viz., to what extent can we employ either lactic acid or its salts as therapeutical agents without inducing mischief? It is not probable that these articles, if exhibited in ordinary doses during health, would produce any very serious disturbance; Nature is so

conservative in her action that such a chemical disposition of them would be made in the economy as to render their resultants innocuous. Nor is it probable that in every form of disease they are capable of exciting any more trouble than they would if administered under normal circumstances; in some disorders, indeed, when wisely administered, they appear remedial in their action. Though this is true, are there not morbid conditions contraindicating their employment? In some maladies the processes of vital chemistry are so disturbed that nearly all the functions are disarranged; now, under such circumstances, a small additional quantity of lactic acid may prove as toxicological as much larger quantities if taken under normal circumstances.

The salts of lactic acid are now very freely employed, and in disorders very diverse in their nature. Are there not morbid conditions which may contraindicate their exhibition if given either in large doses or in small quantities frequently repeated, and extending over a considerable space of time? For example, if an individual has inherited or has acquired a rheumatic diathesis, is it proper to administer to such a person the agents in question? If there is an apparent idiosyncrasy, inimical to lactic acid, is it not more desirable when treating patients possessing such peculiarity, for any malady whatsoever, to administer remedies not containing the acid under consideration? If lactic acid, artificially administered, can excite acute inflammatory action in the fibrous structures of the joints, it is equally liable to induce mischief in the kindred structures of the heart and pericardium. Arthritic inflammation may subside, leaving no permanent trouble in the joints, but, if inflammation is once excited in the delicate cardiac membranes, persistent disaster may be the consequence.

There may be other morbid conditions in which the exhibition of lactic acid is contraindicated, as may occur in those suffering from embarrassed respiration, or in those in whom the functions of the kidneys and skin are seriously impaired. Lactic acid, as the natural product of the system, may, even under such circumstances, be eliminated either *per se* or as

broken into derivatives; if, however, a slight surplus is artificially supplied, the latter may immediately assume the character of a *materies morbi*, while in health such addition would be readily tolerated. Unfavorable symptoms by a superficial observer may be regarded as coincidences, their true import remaining uninterpreted.

It is foreign to my purpose to convey the impression that lactic acid, as ordinarily employed, is inimical to the system, for Nature is so conservative in her action as to antagonize small quantities of materials decidedly poisonous in their character; but the facts noted by Dr. Foster are worthy of our attention, and have excited the inquiries which have been thus briefly set forth.

The Academy then adjourned to September 19, 1872.

STATED MEETING, SEPTEMBER 19, 1872. DR. W. C. ROBERTS, FIRST VICE-PRESIDENT, IN THE CHAIR.

THE SECRETARY read a letter from Dr. E. J. TILT, of London, thanking the Academy for the honor conferred upon him by electing him a Corresponding Fellow.

The VICE-PRESIDENT announced the death of Dr. CHARLES HENSCHEL, at Zurich, Switzerland, September 18th, and of Dr. EMMETT H. KIMBARK, in this city, on the 29th of August, 1872.

Dr. W. C. ROBERTS read a paper entitled "Yellow Fever; is it contagious?" after which—

The Academy adjourned.

STATED MEETING, OCTOBER 3, 1872. DR. E. R. PEASLEE, PRESIDENT, IN THE CHAIR.

DRS. EDWARD FRANKEL, HENRY GRISWOLD, JOSEPH W. HOWE, JOHN W. ROBIE, and STEPHEN W. ROOF, were inaugurated Resident Fellows.

DRS. JAMES ELLIS BLAKE and MAX. J. REINFELDER were elected Resident Fellows.

The President announced the receipt of the *Edinburgh Medical Journal*, for July, 1872, and a list of medical journals in the Library of the Medical Department of the U. S. Army, at Washington; also a supplement to the same.

Dr. A. C. Post reported a case of injuries to the bladder, followed by the formation of a calculus, the nucleus of which was a piece of cloth about one and a half inch long, and three-quarters of an inch wide, from the patient's pantaloons. He had fallen on the end of a stick, which entered the bladder through the rectum, and carried the cloth with it. The calculus was crushed, and removed with the lithotrite.

The President then announced the paper for the evening, of which the following is an abstract:

ON PUERPERAL THROMBOSIS AND EMBOLISM.

By FORDYCE BARKER, M. D.,
PROFESSOR OF CLINICAL MIDWIFERY IN BELLEVUE HOSPITAL MEDICAL COLLEGE.

The paper commenced with the history of a case which occurred in his service in Bellevue Hospital. The patient, a German woman, was brought into the hospital by the police, and found to be in labor. The labor terminated rapidly, after the bladder had been emptied by the use of the catheter, in the birth of a still-born child. She had no hæmorrhage at the time of delivery, and the uterus contracted firmly. Eleven hours after she had a severe secondary hæmorrhage, which was arrested by the use of ice, pressure on the uterus, and ergot. Nine hours after the hæmorrhage she had convulsions—four paroxysms, with but short intervals between each. Repeated examinations failed to detect albumen or casts in the urine. For the following fourteen days she did well in every respect. The fifteenth day after labor she complained of severe pain in the abdomen, which was tympanitic and sensitive to pressure, and also pain in the left thigh. The abdominal pain and tenderness subsided after the action of a cathartic, but the pain in the leg continued the next day, with inability to move the leg. It was found by measurement to be larger than the other, above

the knee, but not below it. She had no phlegmasia dolens, and the pain and difficulty in moving the leg continued but five days. On the eighth day after the attack of these symptoms, she was suddenly seized, while walking in the ward, with very severe dyspnœa. Her surface rapidly became cold, her countenance expressed great anguish, and the pulse was very small and feeble. After the use of ammonia and whiskey she rallied. But the next day her pulse was 132, her respiration 32. She was strictly enjoined to remain in bed, and the bladder was emptied by the catheter. The urine was now for the first time found to be highly albuminous, nearly one-half becoming solidified on applying heat and nitric acid. On the third day after, contrary to orders, she rose from her bed to go to the water-closet, when she fell on the floor, and died in a few moments, apparently asphyxiated. The autopsy revealed dark coagula in right auricle and ventricle, fibrinous and slightly adherent coagula in pulmonary arteries. Both kidneys were highly congested. The vena cava contained a fibrinous clot, which obstructed both renal veins. The left femoral vein contained a pale, firm coagulum, more strongly adherent to the coats of the vessel than that in the vena cava.

Although many of the old writers had noticed these coagulations, and theorized as to their causes and pathological significance, and Van Swieten, more than one hundred and twenty-five years ago, had demonstrated by repeated experiments that, if coagulation takes place in the veins and the clots are carried to the heart, the animals suffer great anguish in breathing and speedily die, yet these facts received but little attention from the medical world until Virchow, of Berlin, made his great pathological discovery relative to thrombosis and embolism. Our late distinguished countryman, Meigs, was one of the earliest to report a case of death from this cause, the significance of which he appreciated, but his theory in regard to the lining membrane of the vein, the endangium, which he regarded as the blood-making tissue, prevented him from really making the pathological discovery which he so nearly approached.

As the terms thrombosis and embolism have been used somewhat loosely, the author of the paper defined them as follows: Thrombosis meaning the arrest of the circulation by coagulation, in either the veins, the arteries, or the lymphatics. The Greek word *embolus* means something inserted like a wedge, something blocking up. Embolism, therefore, implies that the circulation has been blocked up by something transported from some other point, and the nucleus of the blocking agent may be a fragment of a clot, an excrescence from the aortic valves, a pus-globule carried in the circulation until it enters a vessel of too small a calibre to allow it to pass on. So there may be arterial, venous, or capillary embolism, each of which has an important pathological result. It is obvious that, if the embolus is in a vein, it is always carried toward the heart; if in an artery, it is carried in a direction from the heart.

To the late Sir James Y. Simpson belongs the merit of first describing arterial thrombosis as a lesion of the puerperal state. Since the publication of his essay, quite a number of cases have been reported; but it is undoubtedly a very rare affection.

The symptoms of arterial thrombosis are, absence of arterial pulsation below the thrombus; sometimes increased force of pulsation above the thrombus; pain, sometimes very severe, below the seat of the thrombus; coldness of the limb; temporary paralysis of the limb. The cause of arterial thrombosis is no doubt in a large majority of cases an embolus, whose original seat was the heart. In most cases where this has occurred in the puerperal woman, the patient was known to have previously had rheumatism or endocarditis. But in some cases, it seems, the embolus is a clot, which has originally formed in the left side of the heart, for the symptoms of cardiac distress have preceded the signs of local obstruction, while the valves of the heart have been free from disease.

The hæmatologists, Andral and Gavarret, Becquerel and Rodier, have shown that in pregnancy, as well as in various diseases, as cancer, tuberculosis, typhus fever, etc., there is a

peculiar modification of the proportion of the normal elements of the blood, which consists in an excess of the amount of fibrine and serum and a deficiency of the blood-corpuscles, and this condition has been termed hyperinosis. There results from this modification a special tendency to coagulation, termed by Vogel *inopexia.*

Barnes, Hervieux, and the author of this paper, believe that puerperal toxæmia may rapidly develop inopexia, independent of hyperinosis.

Thrombosis or embolism of the pulmonary arteries is a very grave and not a very rare affection of the puerperal period. The thrombosis is sometimes spontaneous or primary, in other cases it is the result of an embolus, the embolus being a clot from a thrombus in the veins. This is no doubt the cause of sudden or rapid deaths, which follow phlegmasia dolens, and sometimes occur in typhus fever, rheumatism, phthisis, etc.

The symptoms of this lesion are great difficulty and anguish in breathing, quick, rapid, feeble pulse, coldness of surface, convulsive contractions of the muscles of the chest, and, in short, the patient dies from asphyxia. Cases were detailed: one a patient of Dr. I. E. Taylor, another a patient of Dr. C. A. Budd, where death took place suddenly, when the patients were apparently convalescent, from phlegmasia dolens. A patient of Dr. Cheeseman, who had previously symptoms of venous thrombosis in the left leg, but did not have phlegmasia dolens, suddenly was seized with symptoms of asphyxia and other signs of embolism of the pulmonary artery. This was followed by evidences of pulmonary infarctions, as pain in the chest, cough, bloody sputa, etc. Her death occurred in the fourth attack.

It is probable that many cases of this character recover by the fragmentation and absorption of the clot.

It was formerly believed that venous thrombosis was generally the result of phlebitis, but the pathological researches of Virchow, Charcot, and others, seem to have conclusively demonstrated that thrombosis is infinitely more frequently the *cause* of phlebitis than the *consequence* of it.

In the treatment of this affection, the paper urged the importance of not hastily giving up the case, the use of carbonate of ammonia and other stimulants, the importance of opiates, equally necessary as in other shocks, and, above all, such conduct and manner on the part of the medical attendant as will inspire hope and confidence, and keep up the *morale* of the patient.

The paper referred to the relation of venous thrombosis, uterine, renal, and pulmonary, and also its bearing on pyæmia, all of which is of great interest in connection with general pathology equally with the special pathology of the puerperal period.

The Academy then adjourned.

Stated Meeting, October 17, 1872. Dr. E. R. Peaslee, President, in the Chair.

The President announced the reception of the *Edinburgh Medical Journal* for October, 1872, and Dr. Yandell's address before the American Medical Association in 1872.

Dr. Gurdon Buck exhibited a patient on whom he had made an operation for the restoration of his nose, bitten off by a drunken sailor. A large flap was taken from the forehead to supply the deficiency at the end of the nose, and a flap from the bridge and base of the nose slid up to partly fill the space on the forehead.

Dr. B. Howard read a short paper on "The Preventable Deaths from Drowning in the Metropolitan District of New York."

Dr. L. D. Bulkley (by invitation) read a paper on "Acne." (Published elsewhere.)

The Academy then adjourned.

Stated Meeting, November 7, 1872. Dr. E. R. Peaslee, President, in the Chair.

Drs. Adoniram B. Judson, Simeon N. Leo, and T. Dwight Martin, were elected Resident Fellows.

The paper of Dr. Bulkley on "Acne" was discussed by Drs. Weisse, Peters, Taylor, Howard, Brown, Bulkley, and Peaslee.

The Academy then adjourned.

Stated Meeting, November 21, 1872. Dr. E. R. Peaslee, President, in the Chair.

Dr. Austin Flint read the paper for the evening, entitled "A Contribution to the Etiology of Pulmonary Tuberculosis." (Published elsewhere.)

The Academy then adjourned.

Stated Meeting, December 5, 1872. Dr. E. R. Peaslee, President, in the Chair.

The Annual Reports were read.

Officers for the ensuing year were nominated.

Dr. Francesco Dichiara, of Palermo, Italy, was elected a Corresponding Fellow.

The President announced the receipt of "The Institutes of Medicine," and "Physiology of the Soul and Instinct," presented by the author, Dr. Martyn Paine.

Dr. D. B. St. John Roosa read a paper entitled "Chronic Non-Suppurative Inflammation of the Middle Ear."

This disease he divided into two forms, catarrhal and proliferous. Proliferous inflammation is that non-suppurative middle-ear disease in which there is scarcely any evidence of disease of the pharynx or of impermeability or swelling of the Eustachian tube. It has been known as sclerosis of the middle ear, or otitis media hypertrophica. The subjective and objective symptoms of these forms of disease were dwelt upon at length. The pathology was illustrated by the reports of Toynbee and others. The causes are such as dispose to inflammation of mucous membranes, and were given in detail. The treatment was then discussed, the paper concluding with an historical sketch of operations upon and through the membrana tympani.

The Academy then adjourned.

Stated Meeting, December 19, 1872. Dr. E. R. Peaslee, President, in the Chair.

The President announced the receipt of the *Edinburgh Medical Journal* and the *Guardian* for December, 1872. Drs. John Ellis Blake, A. B. Judson, Max. J. Reinfelder, Simeon N. Leo, and T. Dwight Martin, were inaugurated Resident Fellows.

Dr. Stephen Smith then read a paper on "Errors of Diagnosis and Treatment of Aneurism."

The Academy then adjourned.

Stated Meeting, January 2, 1873. Dr. E. R. Peaslee, President, in the Chair.

Drs. Charles Rodenstein and F. Le Roy Satterlee were elected Resident Fellows.

The President announced the receipt of a work entitled "Testament Médicale, Philosophique et Littéraire," and several pamphlets from Dr. Dumont, of Rennes, France.

The annual election resulted in the choice of the following gentlemen:

President, Dr. Austin Flint.
Vice-President (to fill vacancy), Dr. W. C. Roberts.
Vice-President, Dr. Samuel T. Hubbard.
Trustee, Dr. E. R. Peaslee.
Committee on Admissions, Dr. E. Eliot.
Committee on Medical Ethics, Dr. E. Krackowizer.
Committee on Medical Education, Dr. A. C. Post.

Dr. C. A. Leale read a report from the Section of Obstetrics and Diseases of Women and Children.

The Academy then adjourned.

Stated Meeting, January 16, 1873.

ADDRESS OF DR. E. R. PEASLEE, Retiring President.

Fellows of the New York Academy of Medicine:

The moment has now arrived which relieves me of the office which your generosity gave me two years ago, but noth-

ing will ever deprive me of a full appreciation and a grateful recollection of the honor thus conferred.

I entered upon my official duties with misgivings, being aware that I have no special talent in that direction. I have performed them according to my ability in the midst of other pressing duties, and not seldom—as any one else would have done—to the neglect of the latter; and I now relinquish them with a twofold pleasure. In the first place, I much need the time for other pressing demands; and, in the second, I have the great satisfaction of seeing next in this chair, one whom I have long recognized as a friend, and who will, I know, perform its duties both efficiently and conscientiously.

I know you will extend to him the same generous support which you have accorded to me. It must always be the fact, in an organization like this, that the scientific labor is performed by a comparatively small number of the members—much smaller, indeed, than should be the case. I have aimed to bring new laborers into the field, but there are still many others who, I hope, will no longer delay to take prominent parts, but will respond to the call to do so by my successor.

On the other hand, the members of the Academy may very much embarrass the presiding officer, though without any such intention—and I now speak from experience. The President must, of course, have a definite programme for at least three or four meetings of the Academy in advance; and, though the Academy has the undoubted right to substitute or interpolate any other proceedings, as it may see fit, it may also sometimes, by introducing a trivial matter to occupy the time, or by voting to adjourn fifteen minutes too soon, entirely break up the arrangements of the President, and embarrass him and those he had engaged to read papers, for several weeks afterward. For example: It must be admitted that the discussion of a paper should occur—unless it is thought necessary to wait for its publication—on the evening of and immediately after its reading. It is, therefore, to be assumed that it will not be postponed without a sufficient reason. But sometimes a member wishes to go home immediately after the reading, and,

looking at his time-keeper, finds it already late, i. e., $9\frac{1}{2}$ or even 10 o'clock—and, therefore, he moves that the discussion be deferred to the next meeting. There are several gentlemen present who are prepared to discuss the paper, but who will probably not be able to be present also at the next meeting. But the Academy votes, almost as a matter of course, to adjourn, and the consequence is, that few if any remarks are made on the paper at the next meeting. As a matter of course also, the same injustice is done, and the same discourtesy committed, to the next reader, i. e., the discussion is again deferred to the following meeting, and under the circumstances actually for want of time.

Now, this may be avoided in two ways: First, if any member wishes to go home after the paper has been read, he may quietly do so, without moving that everybody else shall also be sent home; or, secondly, the President may be asked whether his arrangements for the next meetings will be interfered with by the postponement, and, if so, the Academy may vote down the motion. In my own judgment, the former course would be much the most beneficial to the Academy, since I have observed that the person who moves the postponement of the discussion is almost certain not to manifest any further interest in it, and sure to be absent when it does come off. But, if the motion be made, it remains for the Academy to decide.

One illustration more. Your President has previously arranged for matter enough entirely to fill a particular evening to a late hour, in consequence of the introduction of some extra subject which could not be deferred, when some individual comes here, without any previous notification, with a patient he wishes to show to the Academy—the case being one of interest to himself, at any rate, and to the Academy, perhaps—perhaps not. The President cannot give him the time without injustice to those already engaged, and states this fact, and requests the gentleman to defer his case till the next meeting. The latter then appeals to the Academy, asking for a very short time only, since he does not wish to

trouble his patient to come again; the Academy grants the privilege; the gentleman takes such time as he pleases, and the consequence again is, that the principal paper is deferred, and its reading ended so very late that the discussion must be put over to the next meeting. Again the discussion fails, therefore, and the whole programme is again disturbed for the three or four subsequent meetings.

Of course, a president will treat with courtesy and consideration all the members of this Academy; but both courtesy and consideration are due, *first of all*, to the reader of the paper for the evening; and no other member's claims or convenience should be allowed to conflict with his, or can do so without injustice to him.

I trust, gentlemen, that you will pardon me for these remarks, since they do not demand an increase of power for your presiding officer, but only such an amount of discretion as the best good of the Academy demands.—And now, my friend, will you allow me to induct you to the chair, to which you have been elected with such an unusual degree of unanimity that you need no special introduction here. Your name,* indeed, suggests a nature and constitution very hard and very unimpressible: but we know it is a misnomer in these respects; for, though too rough a friction would certainly make the fire fly, we have always found you the high-minded and sympathetic man, and the genial gentleman, as well as the finished scholar, the distinguished author, and the skillful practitioner.

The Academy has honored you by its preference. I know you—being such—will not fail to honor the Academy, in the chair to which you are now elected; and I present you, for your guidance, the Constitution and By-Laws of this Association.

* Dr. A. Flint.

DR. A. FLINT'S INAUGURAL ADDRESS.

FELLOWS OF THE ACADEMY OF MEDICINE:

A BIENNIAL election offers a fitting occasion for bringing before us the objects of our association, in order to see if any thing can be done to give greater efficiency to the means for their accomplishment. These objects are expressed in the Constitution thus: The cultivation of the science of medicine, the advancement of the character and honor of the profession, the elevation of the standard of medical education, and the promotion of the public health.

First and chiefly, the means for the accomplishment of these objects are the communications, discourses, and discussions at the stated meetings of the Academy. These means constitute the important purpose for which the meetings are held. To endeavor to secure for the successive stated meetings able papers, which, in addition to their intrinsic value, will be useful in calling forth profitable debate, is the most prominent of the duties pertaining to the office which I am to enter upon to-night. It is obvious that success here must depend on the coöperation of the Fellows of the Academy. I shall enter upon my official duties with the belief that I may count upon this essential coöperation. I will add, I have had already abundant reason to anticipate that I shall not be disappointed in this respect.

It is well for us to keep constantly in mind the fact that the great purpose of our meetings relates to the accomplishment of the objects for which the Academy was instituted. All the time devoted to deliberation on irrelevant matters, and on the needless or unduly prolonged consideration of questions which are trivial, or of merely personal interest, is a deduction from the limited period which should be devoted to the great purpose of the meetings. This, as I am aware, is a commonplace truth; but, when our conduct is to be regulated by common-place truths, they can hardly be kept too constantly before us.

I will venture a few remarks with respect to our debates.

It is notorious that, in almost all assemblages, there are some who are over-ready to talk, and there are others who are over-reluctant to have their voices heard. The natural consequence is, the former talk too much, and the latter too little, if at all. It would be strange if the meetings of the Academy were entirely exceptional in this respect. A very simple mode of action, if strictly followed, will prevent redundancy of debate. It is to keep before the mind the inquiry, "Have I any thing to say which will promote the great purpose of the meetings, by the addition of facts, the citation of opinions, or the elucidation in any way of the subjects under discussion." A modification of this inquiry will render it alike a rule of conduct for those who habitually abstain from speaking or speak too little: Can I say any thing which will promote the great purpose of the meeting? Whenever any Fellow can answer this inquiry affirmatively, he is bound to speak. Keeping in view the objects of the Academy is here important. Our organization is not for polemical practice or display. The Academy is not a mental gymnasium or a "debating society." Hence, there should be but one motive in entering into debates, namely, to contribute facts or opinions, and to elucidate the topics under discussion. It is hardly necessary to add, but the fact seems through modesty to be lost sight of by some of the Fellows, that the duty, not to say privilege, of participating in debates belongs to the younger, as well as the older of those who may be in attendance at the meetings.

The importance of making preparation beforehand for engaging in debate is a point to be considered. To be able, without preliminary reflection or study, to do justice to a scientific subject, one must be very familiar with it. In general, it may be said that such a degree of familiarity can only be the result of the attention incident to authorship or oral teaching. With reference to this point, it seems to me desirable that, whenever practicable, ample previous notice should be given of the subjects which will come up for discussion. The custom of submitting views in writing, whenever the previous notice is sufficient to admit of it, should, as it seems to me, be

encouraged. They who have had personal experience in the matter, and all who have had their observations directed thereto, must be satisfied that the pen is a most useful instrument in restraining immature utterances, thereby, and also in other ways, promoting accuracy, precision, and clearness in the enunciation of facts, opinions, and arguments.

In furtherance of the objects of the Academy, it is of course important that the meetings should be well attended. How is this to be secured? Undoubtedly, neither by entreaty, nor scolding. It is certain that a large representation of the Fellows at successive meetings can never be effected by importuning them, generally or collectively, to come, or by reproaches for non-attendance. Nor would it be desirable, were it feasible, to obtain full meetings by such methods. If the meetings can be rendered sufficiently attractive by the papers which are presented, and the discussions which follow, many Fellows will come; if otherwise, few will be present. Largely-attended meetings, therefore, while they are highly desirable with reference to the objects of the Academy, are also evidence of the work which is done for the accomplishment of these objects.

Another point may be touched upon in this connection. It is the desirableness of a punctual attendance at our meetings. Unless the meetings commence at the appointed hour, not only are some subjected to the annoyance of delay, but discussions are sometimes prevented, or prematurely ended, owing to the lateness of the hour. The by-laws require that twenty-one resident Fellows shall be necessary to constitute a quorum. Not unfrequently there is considerable loss of time in obtaining the registration of this number. Anticipating this, some are accustomed to come late, in order to avoid waiting for the business of the evening. Let it be considered that, if they who do this will come punctually, the probability is that the work could begin promptly at the appointed hour, and none would be exposed to the annoyance of waiting.

The publication of papers of interest and value has always been a feature of this institution; and all must admit that important contributions to medical literature are contained in

the printed Bulletins and Transactions which have emanated from the Academy. These publications, however, have had a very limited circulation beyond the Fellows of the Academy; and this has been a serious drawback in the way of their usefulness. A recent improvement consists in having but one publication, namely, the Transactions, into which the Bulletin has been merged. I submit for consideration the inquiry whether it would not be a further improvement to issue the Transactions in a yearly volume. If, among the contributions during a year, those were to be selected for publication which are especially deserving of preservation, they would prove a volume which could hardly fail soon to be in request by the members of the medical profession throughout this country and in other countries. Under these circumstances, to be ranked among the contributors to the volume would become an honor to be coveted; and herein would be an additional inducement to contribute elaborate and able articles. It is certainly a matter of reproach that the metropolis of our country is wanting in a serial of this description; and, with proper effort, why should not the Transactions of the New York Academy of Medicine, after some years, form a series of volumes of which the American medical profession would be justly proud? Why should we not, in this respect, emulate the Royal Medical and Chirurgical Society of London? Any one who has had occasion to consult the long series of volumes which have emanated from this Society, cannot fail to appreciate the pertinency of citing its labors in this direction, as offering to us an example and encouragement. With all due respect to the medical periodical press, were we successful in emulating this Society in the character of an annual volume of Transactions, it would not be the case as it now is, that contributors of papers read at the meetings of the Academy care but little for their appearance in the printed Transactions, preferring rather that they should be given to the medical public in the columns of a medical journal.

There is another matter of no little importance, to which I should not fail to make reference. It is the possession of an ap-

propriate building. This has been considered a desideratum from the organization of the Academy. The by-laws require, as one of the duties of the trustees, that they take charge of the building-fund. A considerable sum for this fund is already acquired. No one can doubt that, from the possession of an appropriate building, many advantages would accrue—advantages, not limited to the Fellows of the Academy, but embracing the interests of the profession at large, both in New York and throughout the country. I shall content myself with this simple reference to the matter, the more because it will be entered into by those far more competent to enforce its claims on the attention of the Academy.

Finally, the Academy of Medicine is one of several organizations in this city for similar or kindred objects. Between these several organizations there should be maintained relations which involve only a spirit of honorable rivalry. It is a well-established axiom that an individual never raises himself by efforts to depreciate a competitor; and this maxim is equally true as applied to associations. Not only are the different organizations laboring for essentially the same objects, but, to a considerable extent, they embrace the same laborers for these objects. To my mind it reflects great credit upon the medical profession of this city, that these organizations are carried on entirely without antagonism, each apparently being pervaded by the catholic spirit of our motto, "*Una fides, altare commune.*"

Thanking you for the honor which you have been pleased to confer on me, and believing that true gratitude is expressed better in actions than by words, I pledge my earnest efforts in behalf of the important objects of the Academy.

The President then announced the paper of the evening, of which the following is an abstract:

THE CAUSES, TREATMENT, AND PREVENTION OF ABORTION.

By A. S. CHURCH, M. D.

Mr. President and Fellows of the Academy:

An abortion is understood to be the expulsion of the fœtus from the uterus before it has attained the degree of development necessary to render it viable.

This period is usually fixed at about the end of the sixth month of pregnancy.

It is not uncommon for a fœtus to be born alive as early as the fourth month, and live for several hours; and cases are reported where fœtuses born at the fifth month, and even at the fourth, have lived and reached maturity, but they are exceedingly rare, and there is usually present the possibility of error in the calculation.

Statistics of the frequency of abortions vary largely by different authors: Collins gives 1 in 66 births; Beatty, 1 in 57; Churchill, 1 in 26; La Chapelle, 1 in 189; Deubel, 1 in 12; in my own practice, 1 in 25.

This great variation is easily explained. Statistics from hospital records show a much smaller percentage of abortions than when obtained from private practice, for the reason that few pregnant women enter the lying-in hospital until after the sixth month, a period beyond which abortions occur. Careful inquiry reveals the fact that many abortions occur early in pregnancy for which the physician is not consulted, so that probably the correct proportion is about 1 in 16 or 18 births.

Abortions occur more frequently during the early months of pregnancy than at any other subsequent time, and it may be laid down as a rule that the shorter the period of pregnancy the greater the liability to an abortion. The twentieth day after conception has taken place, that is, about the time the menses would have returned, and at every subsequent period of four weeks, spontaneous abortions are particularly liable to occur, the danger growing less as pregnancy advances. . . .

The general causes of abortion are as follows: 1. Acci-

dental; 2. Constitutional; 3. Diseases of the uterus and appendages; 4. Diseases of the ovum; 5. Provoked abortions; 6. Abortions from habit.

Under the first head may be mentioned blows upon the abdomen of the mother, causing injury to the uterus, direct injury to the fœtus, or hæmorrhage from partial or complete separation of the placenta; the concussion of falls, whereby the placenta may be detached or the umbilical cord ruptured; injuries arising from certain kinds of labor, exercise, or amusement; violent coitus; coughing; sneezing; vomiting; the use of drastic purgatives, or irritating enemata; mental emotions, and morbid impressions.

Of the constitutional causes and diseases affecting the mother's health, the most prominent of which are the exanthemata, particularly scarlet fever, typhoid fever, and small-pox, a severe attack of either being almost invariably accompanied or followed by a miscarriage; thoracic diseases, by their cough; abdominal, particularly of the bladder and rectum, by their contiguity to the uterus; syphilis; uræmia; hydræmia; anæmia; malaria, and plethora.

Of the diseases of the uterus and its appendages, are acute and chronic metritis; all diseases of the os and cervix, whether inflammation, engorgement, ulceration, a patulous state of the os, injuries to the cervix by unhealed lacerations from a previous delivery, or injuries by the uterotome; cancer; tumors; displacements; adhesions from previous inflammations; voluminous abdominal tumors, preventing the growth of the uterus; excessive rigidity of the uterine fibres and their consequent resistance to growth; and hyperæsthesia of the uterus.

Under the head of diseases of the ovum will be found by far the larger proportion of the causes of abortions. The feeble attachment of the placenta to the uterine wall during the early months of gestation, and its liability to become separated from slight causes, and the extreme tenuity of the membranes and vessels of the early embryo, rendering them liable to numerous accidents, account for many embryotic deaths. A few of the most prominent causes are as follows: Diseases

of the placenta—as atrophy; hypertrophy; inflammation; fatty degeneration; placental apoplexy; embolism of the umbilical vessels from placental apoplexy; separation of the placenta, or rupture of the cord from its being too short, or being wound around the child's neck or limbs; knotting of the cord; diseases of the cord or membranes; dropsy of the cord; amniotic dropsy; effusion of blood between the membranes; syphilis transmitted to the ovum, either through the spermatic fluid or the mother; and other diseases of the parents, as a vitiated spermatic fluid from a father debilitated by debauchery or old age. Nearly all the diseases to which young children are liable have been found to affect the *fœtus in utero* and cause its death.

Provoked abortions are such as are generally resorted to for criminal purposes, or such measures as are adopted by the physician with a laudable object. . . .

In regard to abortions from habit, we meet with women who are in good health, who conceive readily, apparently have no uterine disease, who use all necessary care to preserve the ovum, and still have abortion after abortion, occurring always about the same period of gestation. They come on without any apparent exciting cause, and are very properly termed abortions from habit. . . .

An abortion occurring early in pregnancy, either from habit or from causes destroying the ovum, other than violent, is not usually attended with much trouble. The ovum slips away with but little pain, and usually without much more loss of blood than attends an ordinary menstrual flow; but, if it arises from an accident which disturbs the placental attachment, hæmorrhage becomes a prominent and often an alarming symptom.

Provoked abortions are usually very troublesome. The constitutional symptoms are prominent, the pains are hard, and continue for a day or two, sometimes for several days; the hæmorrhage is profuse, often dangerous; the placenta is frequently retained; metritis, metro-peritonitis, and death, often follow. The explanation (independently of direct injuries to

the patient) why this kind of abortion is attended with such unpleasant results perhaps may be, that the embryo, its attachments, and the uterus, are in a perfectly healthy condition, and the uterus is not easily provoked to expel that which is in no way a foreign body; and, as the embryo is not usually destroyed until it is expelled, and the pains then cease, a portion of the placental attachment remains for some time, allowing a hæmorrhage that is difficult to control.

A complete cessation of the movements of the fœtus, after they have been distinctly felt, absence of the beating of the fœtal heart, a cessation of the enlargement of the abdomen, or a diminished size, a feeling of coldness and weight in the hypogastrium, with flaccid breasts, indicate a dead fœtus, and signs of its expulsion may be encouraged.

The three important symptoms which demand consideration, are pain, hæmorrhage, and an escape of the amniotic fluid.

When called to a patient that has symptoms of threatened abortion, our first duty is to decide whether the patient is really pregnant; second, whether the symptoms are those of abortion; and, if so, is an abortion inevitable? To settle the first point will sometimes require quite as much tact and shrewdness as medical skill. If the pains are without distinct remissions, and precede the hæmorrhage, and are relieved by an increased flow, the canse probably is uterine congestion. If, on the contrary, the hæmorrhage precedes the pains, and the pains gradually increase in severity, with distinct remissions, a miscarriage is imminent. If, on making a digital examination, the cervix is found to retain its normal length and thickness, and the ovum cannot be felt, and the liquor amnii has not escaped, no matter how great the hæmorrhage, there is hope that an abortion may be prevented; but, if the neck has become short, and the os patulous, and if, during the pain, the membranes can be felt to be tense, and protrude, the probability is, that the ovum will escape in spite of efforts to prevent it.

The treatment should be preventive, so long as there is

a reasonable prospect of success. Nervous fears should be quieted, the patient directed to assume the recumbent posture, and each symptom should be controlled as early as possible. Pains should be quieted by anodyne enemata, repeated as often as necessary. When the uterus shows a tendency to cast off the ovum at each pregnancy, owing to abnormal conditions within itself, it may become necessary to use an anodyne enema or suppository every night for two or three weeks, in the mean time, giving attention to the general health of the patient, and the condition of the bowels. Hæmorrhage should be controlled by astringents applied to the vagina or os uteri. The soft sponge, saturated with alum-water or tannin, squeezed nearly dry, and applied against the os, is far more effectual than astringents administered internally. Their internal administration seems to do but little good in arresting hæmorrhage, and considerable harm in disturbing digestion and constipating the bowels. The only internal remedy that I have used with any satisfaction to arrest uterine hæmorrhage is ergot. When the bleeding seems to come from a congested os or cervix, and is not profuse, and even slight disturbance of the placental attachment is suspected, from ten to twenty drops of Squibb's fluid extract of ergot may be given three or four times a day, with satisfactory results. If the patient is plethoric, has been accustomed to a pretty large menstrual flow, and is in the early months of pregnancy, the flow had better not be checked with astringents. A moderate bleeding from the arm will do more to preserve the ovum than any thing else. The recumbent posture should not be insisted upon for more than two or three days. Confinement to the bed soon deranges the health, the appetite is lost, the patient becomes nervous, and I think is much more likely to abort than if she is allowed to be up and take moderate exercise. It is not necessary to enjoin perfect rest until hæmorrhage is controlled. A patient can wear a sponge of the size of an egg, medicated with alum, and be around the house without losing a drachm of blood a day, when several ounces would be lost without the use of this application. I always keep, ready for use, sponges soaked in a

solution of alum, tannin, or acetate of lead, and then moderately compressed. I find them convenient, both for vaginal and uterine application. Those intended for introduction within the os need to be more firmly compressed, smaller, and with the addition of a small quantity of gum-acacia, to prevent a too rapid expansion, and thereby consequent failure of introduction. Hæmorrhage may be so alarming as to require not only the sponge, but a thorough tamponing of the vagina. This, of course, will be likely to cause the death of the ovum by a separation of the placental attachment, if the bleeding comes from the placenta, but there seems to be no choice in the matter, and there is the possibility that a clot may close the bleeding vessels, and the ovum be preserved. A tampon does not necessarily cause the destruction of the ovum.

The hæmorrhage may not come from a separation of the placenta, or any portion of it. It may come from an open vein just within the os, or from an injury to the vagina, and, in either case, the tampon would be successful in arresting the hæmorrhage without much risk to the ovum. We are not always sure, when called to a case of uterine hæmorrhage, that a knitting-needle, intended to be introduced into the uterine cavity, may not have caused simply a vaginal wound, and that the ovum has not been injured by either the instrument or the hæmorrhage, and will not be by its arrest.

In the latter months of pregnancy, if it becomes necessary to tampon the vagina, the effect should be watched, for internal hæmorrhage may take place, and endanger the life of the patient. As hæmorrhage is not, usually, very troublesome during the early months, and, if it is decided that an abortion is inevitable, it had better be allowed to come unassisted, for the ovum may be expelled entire, while any assistance with the finger is pretty sure to rupture the membranes, allowing an escape of the fœtus, with a retention of the placenta, and a troublesome hæmorrhage. An abortion after the third month is usually accompanied with a rupture of the membranes, after which the fœtus escapes readily enough, and the placenta is commonly retained for several days, if no assistance is rendered.

If it can be hooked down with the finger, and removed at once, it is well to do so, but this is not always easily accomplished. If it is detached, it is advisable to make pretty persevering efforts to remove it, and, if the finger fails, Dewees's blunt hook, or a pair of placental forceps, may be found very useful. No harm need be done with either. In some cases, the placenta remains attached to the uterus, and, immediately after the fœtus is expelled, the os closes so firmly as to render it impossible to introduce the finger, except by a tiresome and painful process of dilatation. The hæmorrhage ceases, and the uterus shows no disposition to expel its contents for several days. Under such circumstances, it seems better to wait than to use the force necessary to remove the placenta. I am well aware of the supposed danger of such a course. Hæmorrhage may come on, at a time when assistance is not easily obtained, and the life of the patient be jeopardized, or septicæmia may result from a putrid placenta. I have never seen harm from such delay, and think the danger considerably magnified. I have for the last two or three years, when I found it necessary, or advisable, to leave the placenta undelivered, introduced a carbolized sponge-tent into the os, and tamponed the vagina, and, on removing the tent a few hours after, have succeeded, with but little trouble, in getting the placenta; or, if I still found it attached, introduced another tent, and waited a few hours longer, in the mean time giving a few doses of ergot. In removing the placenta that has not been cast off, either with the finger or an instrument, there is great danger that a portion will remain attached, and keep up a constant hæmorrhage for days and weeks, and cause far more trouble and danger than to adopt the plan just proposed. I think there is more danger, both of hæmorrhage and septicæmia, from the retention of a portion, than the whole placenta; in fact, so long as the plecenta remains entire and attached, there is no danger from putrid absorption, for there is no decomposition. If the placenta is not expelled within twelve or eighteen hours, it will usually remain about a week, when hæmorrhage will set in, and the placenta will be found in the neck, and can be

hooked down without any trouble. Occasionally, a placenta will be retained for several months, without hæmorrhage or any unpleasant symptom. I remember one case when it was retained for three months, and when expelled bore no evidence of decomposition. In one case recorded, the placenta of a five months' fœtus was never expelled. This patient had no hæmorrhage, no lochia, but was troubled with a tolerably profuse leucorrhœa for several months, and general debility. Her health became completely restored, and she has borne several children since.

Besides the danger from hæmorrhage and septicæmia, a retained placenta may become the nucleus for a polypoid growth; therefore, if not expelled within reasonable time, it is better to dilate the cervix by tents, and remove it, than to risk the dangerous consequences of its retention. Ergot used for this purpose will be unsuccessful, except the cervix first be dilated; then, no doubt, it will assist both in controlling hæmorrhage and expelling the contents of the uterine cavity.

The treatment by preventive measures I have only alluded to in cases where symptoms of abortion are already present. The time for successful treatment is in the intervals between gestations, and after pregnancy has taken place, before symptoms have manifested themselves.

To be successful, a thorough knowledge of the history and causes of previous miscarriages is essential. If it depends upon the bad condition of the patient, such measures as will improve her general condition should be prescribed. If from syphilitic influences, appropriate treatment of both husband and wife may become necessary. If from uterine disease, either of the os, cervix, or body, it should receive special attention, and an important part of the treatment consists in requiring total abstinence from cohabitation for several months, not only to avoid the irritation and excitement caused by the act, if the patient is already pregnant, but to avoid pregnancy, and thereby give the organ complete rest, and a system reduced by previous abortions time to rally. If from displacements, they should be remedied by appropriate measures.

If, from excessive sensibility and contractility of the uterine fibres, it is particularly necessary that the organ should have a long rest, and the advantages of change of air, sea-bathing, and the use of arsenic, should be employed. When conception has taken place, there is still time for much to be done, both by treatment and advice.

The plethoric patient can lose a little blood from the arm every month with benefit, at the time when the menses would have appeared. If there is general plethora, it is very necessary; if local, although the general appearance of the patient may contraindicate it, a small bleeding will be well borne, and the danger of miscarriage very much lessened. Patients who have a periodic discharge of blood from the uterus for the first three or four months seldom miscarry. Nature relieves them of the local plethora.

I am not quite prepared to accept Nature's suggestion and apply leeches to the os after pregnancy has commenced, but I once had the good fortune to carry a patient safely through her pregnancy, who had been subject to numerous abortions, by applying leeches to the os after pregnancy had advanced two months, to relieve what I supposed was a local plethora—being unconscious at the time of her pregnant condition. The feeble, anæmic female needs moderate exercise in the open air, and the long-continued use of iron; and, if she suffers from leucorrhœa and a soft, relaxed os, daily astringent injections, or the use of astringent vaginal suppositories. The chlorotic patient will be benefited by the use of iron and the chlorate of potash. An irritable condition of the uturus may need frequent anodyne enemata per rectum. Irregularity of the bowels, either constipation or diarrhœa, needs attention. Syphilis, either in the man or woman, requires a careful and long-continued mercurial course of treatment. I have no knowledge of, or faith in, any special remedies to prevent abortion, except mercury in syphilis. Sauter highly extols the use of savin in fifteen-grain doses, three times a day, for three or four months, to correct the pernicious habit; and Simpson, and subsequently Barker, of this city, are equally as sanguine in the use of the

chlorate of potash in large doses and long continued. I have given the latter remedy a thorough trial several times, but it has failed to meet my expectations. The quantity recommended is so great that I find difficulty in getting my patients to persevere with it. The only cases in which it seems applicable, are anæmic females. To such I still give the chlorate in smaller doses, and in conjunction with iron, with benefit. I am well satisfied that many cases of threatened death of the fœtus from defective nutrition, caused by impoverished blood or by defective placental circulation, either from too small a placenta or fatty degeneration, have by the use of this treatment been carried safely to the full term.

Dr. Fordyce Barker then exhibited a specimen of a perfect and entire ovum, expelled at about the fifth month of gestation, with the placenta attached. The mother, forty-two years of age, had had four children, the eldest sixteen, the youngest seven, since the birth of which she had not been pregnant until the present time. She menstruated August 3d, and supposed that her period recurred again October 3d, missing September, but in October the loss was slight for two days only. November 15th, she again had a loss of blood for two days. As the symptoms which accompanied former pregnancies were absent, she did not believe herself to be pregnant, but supposed that she was suffering from irregularities which were due to a "change of life." Her husband had been seriously ill for ten days past, and she had endured great fatigue and anxiety from this cause. Five days since she began to lose some blood, and complained of pain in the back, but with no symptoms of uterine contraction. She was advised to go to bed, and camphor and hyoscyamus-pills were given to induce sleep, as opium produced in her the opposite effect. At half-past eight yesterday morning the entire ovum was expelled, after an hour and a half of severe labor. The duration and intensity of pain she described as being quite as severe as the labors at full term of gestation. Her husband, who is convalescing from severe illness, when giving his consent to have the specimen taken away, facetiously remarked that he

supposed that the doctor wished to add to his medical bill his services as undertaker.

Dr. Barker then proceeded to discuss, with some minuteness of detail, certain points in the paper of Dr. Church. He was firmly convinced that a healthy ovum is not easily separated from a healthy uterus. One of the very few points in which he should feel obliged to dissent from the author of the paper was, that abortion was ever due to a "habit of the system." The term only covered our ignorance of the real cause of the death and expulsion of the ovum, at a given period in repeated pregnancies. In the progress of science, in those cases where the cause of these repeated abortions cannot be explained by any known condition of the maternal system, constitutional or local, or by any known pathology of the ovum itself, we may yet discover what the real disease of the ovum is. The pathology of the ovum and the placenta is yet to be fully studied. In the five years which had elapsed since he read a paper on this subject, which was published in the *Medical Record*, June 1, 1868, he believed he had acquired some additional knowledge from a careful study of the cases which had come under his observation, but all must admit that the symptoms indicating disease of the ovum are very obscure, and the diagnosis of the disease very difficult. He related one case illustrating this point: A lady, who had five times miscarried between the sixth and seventh month, came to this city from San Francisco, with the anxious hope that she might give birth to a living child in her sixth pregnancy. She was first seen by the doctor on the 16th of September. She had the appearance of perfect health, and the most careful investigation failed to elicit any information which would explain the cause of her previous miscarriage. She was now, as she supposed, about six months pregnant. The sounds of the fœtal heart were very distinct. There appeared to be no indication for treatment, and the patient seemed somewhat disappointed when told this by the doctor. On the 28th of September he was again called to see her, and found her extremely despondent, saying that she felt exactly as she did when her previous

miscarriages occurred, but she had no symptoms indicating such a danger, except that her countenance was anxious and worn, her pulse was quick, and her temperature 100°. The sounds of the fœtal heart were distinct. The next morning her moral depression was great, her pulse 112, her temperature 103°, and her tongue white. The sounds of the fœtal heart were feeble, and too rapid for him to count. But she had no hæmorrhage, no uterine pain, but an obscure, ill-defined pain and weight over the lumbar region. Abdominal palpation gave entirely negative results. The urine had been examined by Dr. Flint, Jr., and pronounced normal. A vaginal examination revealed no abnormal condition of the cervix, but pressure upon the posterior wall of the uterus caused pain, and the rectal touch gave a peculiar boggy sensation to the finger, and caused faintness and nausea. These were absolutely all the signs and symptoms that could be ascertained. Placentitis was suspected, and the patient was with some difficulty persuaded to submit to venesection. About sixteen ounces of blood were taken from the arm, and it was to him very interesting to remark that after the bleeding the sounds of the fœtal heart were much more distinct and were easily counted as 140 per minute. She was kept in bed for a few days, but afterward she was advised to go out and take exercise as much as possible, short of great fatigue. She was given chlorate of potash in twenty-grain doses, three times a day. She took eleven ounces of the chlorate between this time and the period of the labor. December 28th she was delivered of a perfectly healthy girl, weighing six and one-third pounds. It was very thin, but vigorous, and has grown fat rapidly since birth. On examination of the placenta, there was a colorless fibrinous deposit, nearly two inches in diameter, in the centre of the placenta. There had evidently been a circumscribed centric placentitis.

As to other points in the paper, Dr. Barker could not speak from personal experience as to the use of ergot as a prophylactic against abortions, but many writers concurred with the author of the paper in recommending it for this purpose. He

never used the ergot to facilitate the expulsion of a portion of retained ovum, but chiefly used it to promote involution in those cases of passive hæmorrhage which are sometimes met with after abortion. He had used the ergotine hypodermically for this purpose. His experience had not been favorable in using this agent in alcoholic solutions, as recommended by some German writers, as Hildebrandt, Drasch, Langenbeck, as the injection caused severe local pain, and in two instances was followed by troublesome abscess. Dr. Barker used ergotine ℈ ij; aq. puræ, glycerine, āā ℥ ij—six drops then contain one grain of ergotine. But, on the whole, he was disposed to believe that he got just as good results from the ergot when used as rectal enema, two drachms of Squibb's fluid extract, with an ounce of thin starch or arrow-root, thrown and retained in the rectum. He agreed with the author of the paper in the belief that the mistake was sometimes made of keeping the patient too long in the recumbent posture, to prevent a tendency to abortions. Every thing which enfeebles the general health increases the liability to abortion.

He referred to the error of assuming that the ovum is dead from the amount of blood lost, the use of opiates, astringents, the tampon, and other measures, and concluded by expressing his assent to most of the doctrines advocated in the paper which had been read, and his appreciation of the author's honest, conscientious work.

The Academy then adjourned.

STATED MEETING, FEBRUARY 6, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

DR. W. B. LEWIS was elected a non-Resident Fellow.

The PRESIDENT announced the death of DR. JAMES L. BROWN, on the 4th inst., aged forty-two years, and appointed a committee to present suitable resolutions.

Dr. CHURCH's paper on the "Causes, Treatment, and Prevention of Abortion," was then discussed by Drs. PEASLEE CARO, CHADSEY, and ROBERTS.

The Academy then adjourned.

STATED MEETING, FEBRUARY 20, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

THE PRESIDENT announced the reception of the "Transactions of the Royal Medical and Chirurgical Society of London," vol. lv., and proceedings of same Society, vol. vii., part xxxv., the *Edinburgh Medical Journal* for January, 1873, and *Verhandlungen der Physikal Medicin Gesellschaft in Wurtzburg*, as contributions to the library.

Dr. H. D. NOYES then exhibited, with the magic lantern, a large number of illustrations of the retina and optic nerve, in health and disease.

The Academy then adjourned.

STATED MEETING, MARCH 6, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

THE PRESIDENT announced the reception of two copies of the "Report of the Supervising Surgeon of the Marine Hospital Service of the United States," for 1872, presented by Dr. J. M. Woodworth, and the *Edinburgh Medical Journal* for February, 1873.

Dr. C. A. LEALE then read the paper of the evening, entitled

OBSERVATIONS ON THORACENTESIS (THORACOCENTESIS—PARACENTESIS THORACIS).

BY CHARLES A. LEALE, M. D.

MR. PRESIDENT AND FELLOW-ACADEMICIANS:

FROM the earliest history of surgery, we read of the operation of opening into the thorax for the removal of the products of disease; and yet there are patients permitted to die in this our advanced state of surgical knowledge, after having been bled, purged, and blistered, in vain attempts to produce absorption, rather than be saved by thoracentesis, which, as I will this evening endeavor to show by recorded cases occurring during the past eight years, not only rescues the patient

from immediate death, but can also be the means of commencing a convalescence which may end in the restoration to perfect health.

Surely, in some instances, we might exclaim that, in the multiplicity of the new, many old and valued precepts are neglected.

The history, in brief, of thoracentesis, commences with Hippocrates and his followers, who, in purulent collections, used the knife, the caustic, and the hot iron, to open the chest and give exit to the pus.*

Ambroise Paré has the credit of having been the first to use the trocar and canula for the withdrawal of fluid from the thoracic cavity.†

In 1624, Gerome Goulu alleged that he succeeded more frequently in puncturing the chest for hydrothorax than when he performed abdominal paracentesis for ascites. ‡

In 1658, Bontius, for the first time, took up in a precise manner the subject of allowing air to be introduced into the chest. §

In 1841, Prof. Skoda, and Prof. Schuh, of the Vienna School of Medicine, published an important work on this therapeutical subject. ‖

In 1844, Prof. Trousseau read a memoir before the Academy, on paracentesis thoracis. ¶

During the past twenty years, Dr. Henry I. Bowditch, of Boston, has operated on at least one hundred and fifty-four persons, making two hundred and fifty tappings—more times, perhaps, than any other man in this country.**

In Dr. Bowditch's first case, the trocar and canula of Dr. Morrill Wyman were used, with a suction-pump attached, but

* "Œuvres complètes d'Ambroise Paré," par J. F. Malgaine, tome ii., Paris, 1840.

† Lectures on Empyema, by Mr. Guthrie, London *Lancet*, June, 1853.

‡ Trousseau's "Clinical Medicine," vol. iii., English edition,

§ Ibid. ‖ Ibid ¶ Ibid.

** Bulletin of the N. Y. Academy of Medicine, vol. iv., No. 1.

since that time Dr. B. has devised an instrument, his modification of the trocar and canula, by which he thinks the operation is rendered less painful.

In January, 1865, Prof. Austin Flint, at his Bellevue Hospital clinique, removed over two pints of pus from the chest of an adult male, by a very simple apparatus devised by himself. It consisted of trocar and canula, about six inches in length, and corresponding in diameter to a No. 12 catheter (English measurement). To the end of the canula was attached one of Davidson's India-rubber syringes, which was manipulated and the quantity mentioned withdrawn while the patient was quietly sitting upright in a chair.*

In the spring of 1865, I operated by incising the intercostal space with scalpel, then introducing a No. 8 male silver catheter, to the end of which a Davidson's India-rubber syringe was fastened, and withdrew sixty-eight ounces of exceedingly offensive pus; closed wound hermetically; no more pus collected in chest; and, four years afterward, heard that the patient was enjoying good health. The history, in brief, is as follows:

Case I. *Removal of Sixty-eight Ounces of Pus from Chest; Wound closed; Recovery of Patient.*—Captain N. B., One Hundred and Forty-second Pennsylvania Volunteers, aged twenty-eight years; wounded at Petersburg, April 1, 1865, by a conical ball, which entered one-fourth of an inch to the left of the ensiform cartilage; passed beneath ribs, through lung of right side, making its exit at the eighth rib on a line below the axillary space. Hæmoptysis followed, and lasted for several hours.

29*th.* — He was admitted to officers' ward, Armory Square Hospital, Washington. On physical examination, I found the two lower lobes of right lung collapsed, and partial solidification of the upper lobe. There was considerable febrile movement, he had become very much emaciated, and expectorated a very large quantity of the usual rusty sputa.

* "Private Notes of Clinical Cases," by C. A. Leale.

One ounce of champagne and one ounce beef-extract were given each alternate hour while awake, and the severe pleuritic pains controlled by the hypodermic use of morphine.

May 6th.—While examining him, I easily produced the splashing sound, when shaking him suddenly. The line denoting the height at which the fluid came, while in the sitting posture, was about two inches above the right nipple. Not wishing then to operate, I left the ward, seeing him twice a day. The pus rapidly accumulated, and in two or three days I was hastily called to see him, the nurses stating that he was gasping for breath, and apparently dying. On arriving at the tent where he had been transferred, to get the benefit of fresh air, I found him sitting up in bed supported by two nurses, gasping agonizingly for breath. Both wounds made by bullet had entirely healed.

I immediately made a valvular incision near the eighth rib; introduced a No. 8 male silver catheter, so that its extremity nearly reached the diaphragm (about four inches below the opening). To the free extremity of the catheter a Davidson's syringe was easily attached, when, by slow suction, sixty-eight ounces of thick, decomposing pus were withdrawn.

The stench of the liquid was so intolerable that long before the termination several of the attendants became faint and left. The catheter was now withdrawn, when the integument was firmly held over the valvular opening, and the wound hermetically sealed. The operation had not terminated fifteen minutes before he expressed great relief, and from that time he steadily improved in health.

26*th.*—Twenty days after the operation, before leaving for his home, I examined him, finding that he was rapidly regaining the use of his lung. He then easily walked a mile, and was apparently in a condition for a speedy and complete recovery. In answer to a letter from me, four years after being wounded, he stated that he was enjoying excellent health.

CASE II. *Traumatic Empyema, free Opening for Drainage of Pus; Death from Pneumonia of Opposite Lung.*—

Captain J. N. H., Two Hundred and Tenth Pennsylvania Volunteers, aged twenty-two years. March 31st, at battle of South Side Railroad Station, received a penetrating gunshot wound of right thoracic wall. April 12th, came under my care; had collapse of right lung. At the time of injury he had slight hæmorrhage from wound only; I found that the ninth rib had been broken, that pus had accumulated in the chest, the wound having nearly closed. Several spiculæ of dead bone from the rib I removed, being sources of continued irritation; there was then very little liquid in the chest; soon, however, the wounds healed, and it was found that the liquid increased in the chest to such an extent that a bulging two inches below the wound appeared; this I opened by a free incision, allowing a free exit. He commenced to improve, but pneumonia of opposite lung ensued, causing his death, May 5, 1865.

Case III. *Perforating Gunshot-Wound of Lung, where considerable Air and Blood were hermetically sealed up in the Chest.*—This patient returned to duty in less than five weeks, and in nineteen months afterward was taken by me to the offices of Prof. F. H. Hamilton and Prof. Austin Flint, each giving him a careful examination.

Thomas Conaughton, private, Company F, Fifty-sixth New York National Guards, aged nineteen years, while in perfect health was accidentally shot by his comrade who was carelessly handling his Enfield rifle, while on duty at prisoners' camp, Elmira, at 10 o'clock P. M., August 13, 1864.

The ball entered half an inch to the right and below the nipple, passing through the right lung, making its exit at the margin of the posterior border of the inferior angle of right scapula; it then passed through the arm and chest of a friend, who, in consequence of paralysis, survived only a few days. The muzzle of the gun was within six inches of Conaughton's chest at the time of its discharge. The velocity of the ball was so great that, after having passed through the bodies of two men, it perforated a knapsack filled with clothing, and was lost. Conaughton soon became faint from loss

of blood, which flowed freely from his mouth. He was immediately attended by the surgeon and assistant-surgeon of his regiment, who applied water-dressings; in half an hour whiskey was given, causing coughing to commence, and, at each severe effort, profuse hæmorrhage from mouth. At 1 o'clock A. M., August 14th, three hours after the accident, he was brought to the United States Army General Hospital and placed in my charge. He had great dyspnœa and several attacks of syncope, blood and air being drawn in and expelled from wound during his violent efforts to cough.

I immediately removed the dressings which had become misplaced, and, on examining the wounds, found that the edge of the rib at point of entrance of ball had been denuded of its periosteum, but not broken; as soon as the dressings were removed, quite a quantity of air entered the chest through the wound with a shrill, hissing sound, causing a return of the dyspnœa and syncope; he ceased breathing for a moment, and at each effort to inspire increased the admission of air into the chest. In an instant I slid the integument over the opening for at least an inch, thereby closing wound completely; his breathing now was regularly established, but soon violent paroxysms of coughing followed, when simultaneously air and blood were with great force expelled from the anterior wound. I then performed Dr. B. Howard's operation of hermetically sealing, first by removing the uneven edges, uniting by silk sutures, then covering all with court-plaster, and six or eight coats of collodion, and over all a moderately firm bandage was applied; ʒ ss tr. opii was given, and in less than an hour he was asleep in the semi-recumbent posture in bed. At 8 o'clock A. M. I saw him, and found that he had passed a very comfortable night; he had considerable febrile movement and commencing pneumonitis; the cough continued, and for four or five days he raised at least eight ounces of frothy, bloody sputa; at times the blood came away in mouthfuls, and was of a bright, arterial color, occasionally containing coagula. The dressings dropped off in three days, the anterior wound having entirely healed, and at the posterior wound

there were two or three drops of pus from the integumentary surface only; the pneumonia lasted for two weeks, and in less than five weeks from the time of being wounded he was returned to duty in his regiment. Being now ordered to a different station, I lost sight of him until the following February, when I was requested to examine him, while he was reënlisting, finding him in excellent health. On the 19th of March, 1866, over a year and seven months from the time of reception of wound, I presented him at the office of Prof. F. H. Hamilton, where both Dr. Hamilton and Dr. Elisha Harris examined him: externally nothing could be seen except cicatrices and marks of the sutures; at the request of Prof. Hamilton, Prof. Austin Flint gave him a thorough physical examination, and found slight dullness, confined to a space about four inches in circumference, due to pleuritic adhesions; otherwise that lobe was perfectly healthy, as were the remaining lobes of both lungs. His general health was perfect.

Case IV. *Traumatic Empyema; Free Opening for Discharge of Pus; Death from Exhaustion.*—Colonel C. R. P., Sixth Maryland Volunteers, aged thirty years, gunshot-wound of left thorax, ball entering at ensiform cartilage of sternum, passed anterior to mediastinum, fractures the sixth rib, passing between the pleuro-costalis and pleuro-pulmonalis, making its exit three inches below the nipple.

May 14th, I found that the fractured extremities of ribs were undergoing necrosis; he had frequent attacks of dyspnœa, and occasionally syncope. Morphine was given hypodermically to relieve pain. He had champagne, and the best diet carefully given. The necrosis soon caused several roughened edges on the ribs, immediately covering the heart, and being fearful of perforation of the pericardium, which could easily be seen, immediate consultation was called, and Surgeon Basil Norris, U. S. Army, skillfully removed all dead bone, the patient being profoundly anæsthized by chloroform for over fifteen minutes. As he depended wholly on respiration by opposite lung only, the anæsthetic was more slowly given than usual, and after the termination of the operation his return to con-

sciousness and regular breathing was very prolonged. The patient rallied well, and by the operation a free outlet of pus was made. The discharge now became exceedingly offensive; this was controlled by the assiduous care and skill of Dr. C. B. Porter, who washed out the cavity with a diluted solution of the chlorinate of soda. The patient slowly gained strength, even during the very hot months of July and August, and, contrary to advice, left Washington in a drawing-room car. The journey caused such exhaustion that he died a few hours after arriving at his brother's home in Brooklyn.

Case V. *Traumatic Empyema; Pus discharged by a Free Incision; Recovery.*—Colonel B. F. S., Tenth New York Cavalry, aged twenty-nine years. At the battle of Dinwiddie Court-House, March 31, 1865, received a perforating gunshot-wound of left thorax. On (or near) April 12th, came under my charge. The wound of entrance was six inches below the left nipple; the wound of exit near the spine, at eleventh rib. The wounds had closed spontaneously, although the tenth rib had been fractured, pus rapidly accumulated in chest. I made an incision three inches in length, removed several portions of necrosed bone from fractured rib, giving exit to about a pint of offensive pus; then washed out cavity with diluted liquor soda chlorinate.

Patient has valvular insufficiency, with hypertrophy of heart, which he stated had troubled him for several years.

May 4th.—There was a fistulous opening, through which a small quantity of pus each day was discharged (about ℨij). He still had occasional attacks of dyspnœa, but was well able to return to his home by rail.

Case VI. *Traumatic Empyema; Free Opening for Continued Discharge of Pus; Bullet remained in Lung; Recovery.*—Colonel A. B. F., Sixteenth Maine Volunteers, aged twenty-five years, gunshot-wound of left lung at battle of Five Forks, April 1, 1865. The ball entered between fifth and sixth ribs lateral aspect of chest; remaining in parenchyma of lung.

May 7th.—Admitted to my care; found empyema, coupled

with pneumonia. At his request, I enlarged the opening, and carefully used the Nélaton probe, which came in contact with a hard substance, after being introduced four inches, but, as not the slightest stain could be detected on its withdrawal, it was supposed that the ball might might have become encysted; and, as on previous examinations he had had two severe attacks of hæmoptysis, it was deemed advisable to let it remain. He gained strength rapidly, and June, 1865, returned home, pus still discharging through a fistulous opening.

August, 1866.—In answer to a letter from me, he stated that the ball was in his lung, that the opening had long since closed, and that he had moderate good health.

The following case shows how the pleura may be pushed considerably from the normal position, as has been sometimes noticed when empyema has been operated for by means of the trocar:

Case VII.—General B. W., aged thirty-three years; wounded at Petersburg, April 2, 1865. The ball entered at tenth rib, right side, passing beneath ribs without perforating the pleura, making its exit one inch to the left of first lumbar vertebra, after having passed between the spinous processes. The wound suppurated along its entire course; the sloughing tissue in about a week was entirely removed by a seton of oakum, after which pressure soon caused wound to heal. May 4th, returned home in good health.

Case VIII. *Traumatic Empyema where a Free Incision was made for Exit of Pus; Death and Autopsy.*—Captain P. D., Two Hundred and Eighth Pennsylvania Volunteers, aged twenty-six years; wounded at Petersburg, April, 1865; came under my care April 24, 1865. He was wounded while on horseback, during a retreat, by a conical ball, which entered beneath the inferior angle of the left scapula, passing through the left lung, making its exit at the apex of the clavicular border of the deltoid muscle of left arm.

He told me that, at the time of being wounded, he was leaning forward on his horse as much as possible, holding the reins in his left hand, and fleeing from the enemy. When I

first saw him, he had great dyspnœa, and very severe pleuritic pains, which were terribly increased by a violent cough. The wound of entrance had healed, wound in arm suppurating, considerable pus had collected in his chest, and during a severe expiration a gurgling sound was produced by air being forced through pus from bronchi in thoracic cavity. The sound produced was so loud that it could be easily heard a distance of thirty feet, and several times, during very severe attacks of coughing, the air and pus would be thrown through wound near shoulder.

May 9th.—By a free incision I reopened and enlarged the wound of entrance, and twelve ounces of pus escaped. The opening was kept free, and the chest, which was very offensive, carefully washed out daily, by a dilute solution of the chlorinate of soda. To control pain, it was found necessary to give as much as one grain morphine, hypodermically, at each dose. Every thing that he called for, in the way of nutritious diet, was given him, and also eight ounces of champagne daily.

He soon commenced to improve, the cough diminished, the opposite lung easily performing the necessary respiratory function, and he was congratulated on his prospect of a speedy return home, which, however, unfortunately never came, for on the 2d June I was hurriedly called to see him, and found that during a violent paroxysm of coughing he threw up his arms, and, before his attendant reached him, expired.

At the necropsy, I found the chest nearly filled with blood, which had come from a slough in the axillary vein in course of wound.

Case IX. *Empyema.*—Boy, aged twelve years; chest opened by incision, and pus allowed to flow out; made a good recovery; opening closed in six weeks.

Case X. *Empyema.*—Boy, two years old; chest opened by incision; eighteen ounces withdrawn; wound healed in four weeks; complete recovery.

Case XI. *Empyema.*—Girl, nineteen months old; chest opened by incision. This child was first brought to my class for diseases of children, at the Northwestern Dispensary, Feb-

ruary 14, 1870, her mother saying that she had been in poor health for two months; that the little one cried from pain whenever her chest was compressed while being lifted. On examination, there was dullness over entire right lung; the left lung was in good condition. On the following day I made an incision between the seventh and eighth ribs, introduced the silver catheter, and, with a Davidson's India-rubber syringe, removed sixteen ounces of thick pus. During the operation the pain was very slight, and after the removal of the pus she felt much better. The wound, which was valvular, was easily closed by three fine silk sutures, then covered by a piece of court-plaster. On visiting her forty-eight hours afterward, the plaster was found to have dropped off. The stitches were removed. Wound entirely healed.

September, 1870.—She was brought to my office, her mother saying that during the past summer she had had several severe attacks of diarrhœa, from which she recovered. The child then was in excellent condition, and on examining the right lung all the lobes were resonant, performing the necessary respiratory function, there being no reason to doubt her entire recovery.

Fourteen months after the operation of thoracentesis the child died of acute enteritis.

Necropsy, assisted by Dr. Howard, on the following day. Body well nourished. As there had been no noticeable abnormal condition except the inflammation of the bowels, the abdominal and thoracic cavities only were examined. No peritonitis; the mucous membrane of the bowels presented the appearances of recent acute inflammation and cause of death. The left thorax and contents were normal. On removing the right lung it was found to be adherent, at several points, by pleuritic bands at the superior and posterior border; it was inflated. Over its surface were visible several small deposits of fibrinated lymph. All the lobes were inflatable. On cutting into the parenchyma, the only abnormal conditions existing were four or five portions, about the size of small nutmegs, of simply compressed lung. There were no cheesy or tubercular deposits found in any part of the body.

CASE XII. *Thoracentesis, and Removal of Eighteen Ounces of Pus, for the Relief of Empyema in a Boy Twenty-two Months old; the Wound was left open, and a Year afterward Pus was still discharging through a Fistulous Opening.*—M. M., grandchild of a New York physician—parents predisposed to pulmonary trouble. The child was in good health until the early part of November, 1869, when, after a severe chill, pleurisy, with effusion, developed. December 5th, I was hurriedly called to see him for the first time, and found him sitting in his mother's lap, with cyanotic countenance, rapidly breathing, and his body bathed in a cold, clammy perspiration. The accumulation of liquid in the the thorax was so great as to cause complete relaxation at the sterno-clavicular articulation. The pressure from within was so much that the usual depressions at the intercostal spaces were lost, and the entire affected side presented a strikingly bulging rotundity. The suffocation was so near that he would lose his breath, and with difficulty return to regular breathing again. I was afraid that he would die before I could open the chest. This, however, was immediately done at the eighth rib, on a line below the angle of the scapula, when eighteen ounces of very thick pus escaped. As soon as the pressure had been removed, the little child experienced such relief that regular breathing was soon established, and he fell asleep. The wound was dressed daily, with a pad of oakum, moistened with water, which permitted a continuous drainage. In a month, the discharge was hardly perceptible, and I endeavored to close the fistulous opening by stimulating its edges with nitrate of silver, but this proved of no avail. One year after I had operated, I examined the child, and found that the fistulous opening still existed, but that it had closed several times during the interval; that each time it closed the child would cough, and have considerable febrile movement, all of which left as soon as a free exit of pus was established.

The child's general condition was not good. The lungs were the seat of a low grade of chronic inflammation. The

distal phalanges of all fingers and toes were twice their normal size, the extremities presenting the appearance of greatly impeded pulmonary circulation, being cold and blue, with veins continually distended.

The child was taken to the country, and has not since been heard from.

CASE XIII. *Great Relief from Pain, and Prolongation of Life, following the Removal of Sixty Ounces of Pus from the Chest of an Adult having Phthisis.*—Miss Annie F., aged twenty-one years, had phthisis pulmonalis, and in consequence had not menstruated for two years.

On June 23, 1868, I was hastily summoned, for the first time, and found her supported by friends in the sitting posture, in bed, gasping for breath, bathed in cold perspiration, and suffering from intense, lancinating, pleuritic pain.

On examining the chest, I found complete dullness over entire surface of the left lung, and was easily convinced that there was a large collection of pus in the thoracic cavity; this, with the impaired use of the opposite lung, caused her to be in the greatest misery, and apparently dying in great agony.

By a very small, and almost painless incision with the scalpel, I opened the chest on its lateral aspect, at the seventh rib, introduced through the opening a No. 8 male silver catheter, at the end of which a Davidson syringe was attached, when sixty ounces of offensive, thick pus were slowly withdrawn. During the suction, especially when about a quart had been removed, she experienced such relief that she thanked me for having operated, but, as the operation proceeded, she had occasional lancinating pains, and slight attacks of syncope, which were easily relieved, by giving half an ounce of brandy, and discontinuing the suction for a moment. At the termination of the operation, she was so much relieved, that she could respire quite freely, as long as no air was permitted to enter by the wound. Two or three times during the operation, and on the withdrawal of the catheter, a small quantity passed in, producing such dyspnœa as to prove

conclusively that she could not tolerate a free incision, for the free admittance and exit of air.

The wound was, therefore, hermetically closed, and in three days had entirely healed.

She regained her strength so rapidly that in one week I found, on visiting the house, that she had gone to Central Park (which was just opposite), and for the several succeeding days following took a short walk in the garden. She continued to recuperate until July, when the weather became intensely hot and she again gradually lost strength, until July 12th, when she died an easy death, by exhaustion. No autopsy was permitted.

She was apparently dying when I first saw her, while suffering intense pain, and there is little doubt but that the removal of nearly half a gallon of pus relieved pressure on her right lung, prolonged life, gave great ease, allowing her, when she finally became exhausted, to die comparatively free from pain.

Case XIV. *Spontaneous Cure of Empyema, by Discharge per Bronchi.*—Niemeyer states that "a penetration of the empyema into the lungs, and its discharge by way of the bronchi, sometimes happen," but that a recovery in such instances is rare. In my notes I have the account of one such, where as much as thirty ounces was ejected in a single day, yet in sixteen months afterward the patient was in good health. The history in brief is as follows:

Captain T. A. H., Ninety-first Pennsylvania Veteran Volunteers, aged twenty-six years. Penetrating gunshot-wound of lung; ball entered three inches above the nipple, right side, and lodged in the parenchyma of the lung; he had considerable hæmoptysis shortly after reception of wound. April 29, 1865, he was admitted to my ward; the wound had entirely healed; he had extensive empyema. About the middle of May following, during a violent paroxysm of coughing, he was nearly strangled by a large quantity of pus, which ran from his mouth. Knowing that it must have come through the bronchial tubes, I requested the nurse to save all that came away in the

following twenty-four hours, and found it measured thirty ounces. His attacks of pleurodynia were relieved by morphine. In less than a week after the discharge of pus by mouth he commenced to improve, and so continued until he left Washington. I heard from him a year and four months afterward, when he stated that he was entirely well, and an active business-man in Philadelphia.

Dr. George A. Otis, U. S. Army, in his review of the operation of thoracentesis, as practised during the war, states that the point of election for the first puncture, though in a measure determined by the seat of the injury and the nature of the effusion, appears to have been the seventh intercostal space, one-third of the distance from the spinous processes of the vertebra to the median line of the sternum. This point was selected in nine of seventeen cases in which this particular is noted. In five the puncture was made between the eighth and ninth ribs, and once in the fourth, once in the fifth, and once in the tenth intercostal space. The ordinary trocar, furnished in the field operating-cases, was usually employed; but in a few instances the methods and apparatus recommended by Drs. Wyman and Bowditch, and by Dr. Flint, were employed.*

In a bibliographical notice of the St. Thomas's Hospital Reports,† Dr. G. H. Evans presents tables clearly showing the advantage of performing the operation early in the disease. Thus, out of a total of 533 cases, 373 recovered, 153 died, and in 7 the result was doubtful; this makes the rate of mortality 29 per cent. But out of a total of 308 cases, in which the effused fluid was serum, 74 died, making the rate of mortality 24 to 26 per cent. In 24 cases of serous effusion, in which the operation was performed not later than the end of the fourth week, 21 recovered, and 3 died; in one of the

* "The Medical and Surgical History of the War of the Rebellion," part i., vol. ii. Surgical History, prepared under the direction of Joseph K. Barnes, Surgeon-General U. S. Army, by George A. Otis, Assistant-Surgeon U. S. Army.

† *American Journal of Medical Sciences*, October, 1872.

three fatal cases there was disease of the liver, and in the other two the operation was performed as a last resource, merely with the view of prolonging life. In 17 cases where the operation was performed after the first month, but not after the second, 13 recovered and 4 died; one of these latter died two years afterward, of phthisis. In 10 cases where it was after the second month, but not after the fourth, 5 recovered, and 5 died. In 8 cases, where the effusion was more than of four months' duration, 3 recovered, and 5 died.

In a report on "Scandinavian Medicine," by Dr. J. W. Moore,* a monograph by Dr. L. F. Toft, of Copenhagen, is noticed, in which he deals with thirty-seven instances of spontaneous primary empyema, which term he applies to those cases where from the first a pus exudation is the consequence of inflammatory action. In addition, fifty-one cases are referred to, in which the original purulent nature of the exudation could not with certainty be ascertained. In both classes the proportion of male subjects of the disease was double that of women similarly affected.

Of the 37 examples of primary empyema, 25 occurred in men, and only 12 in women; of the 51 cases of empyema, in which absolute purulent origin was not a necessary factor, 36 occurred in men, and but 15 in women. The disease was most frequently met with between the ages of twenty and forty years. The effect of season was clearly shown by the statistics of all the cases; the greatest number of patients being affected during the winter months, and again in April, when the changes of temperature were excessive, and there was greater exposure from renewed work in the open air.

As regards the seat of the effusion, it was double in four of the whole eighty-eight cases, on the right side in forty-seven, and on the left side in thirty-seven instances, the respective percentage being 4.6, 53.4, 42.0.

In his analysis of the physical signs of empyema, Dr. Toft remarks: "Metallic tinkling appears to arise in two different ways, both of which are dependent on the presence of air in

* *Medico-Chirurgical Review*, July, 1872.

the pleural cavity. Either a bubble of air presses out from the opening into the lung, and bursts in the pleura, or a drop falls from its roof down into the fluid below. The second mode requires more room, and less fluid than the first, the bursting of an air-bubble is caused by or follows a respiratory effort, while the fall of a drop depends on the movements of the body."

In an article on thoracentesis by suction, in empyema and hydro-pneumothorax, by Dr. Bouchut,* after mentioning the old "Galenic plan of simply opening up the cavity of the chest by an incision in the intercostal space, and the modification, in which a Chassaignac's drainage-tube was introduced, he proceeded to describe his own plan, which consists of frequently-repeated respirations, with Dieulafoy's pneumatic respirator, often repeated two or three times in the week. He believes this method to be far superior to any other, but points out that, if the lining is bound down by adhesion, it will not expand to take the place of the fluid removed, and that then it may be necessary to make a counter-opening, and introduce a drainage-tube. This method, however, evidently involves a protracted treatment, for in the three cases brought forward by the author, as illustrative of the advantages of his method, one was still under treatment after nine months' continuous operation; another was discharged cured, after six months' treatment and thirty-nine operations; while the third case ended fatally."—*Gazette des Hôp.*, January, 1872.

In a paper on thoracentesis, in the *English Practitioner* for August, 1872, Dr. Clifford Allbutt states that, during the past three years, he has advised thoracentesis in fourteen cases, and with but one doubtful result. In many cases, as much as one hundred or one hundred and twenty ounces were drawn off. Indeed, the doubtful case, he states, was doubtful only in appearance, and, so far as the operation was concerned, it may justly be reckoned among the successful ones. Dr. Allbutt cites a remarkable instance, which he saw in consultation with Mr. Smith, of Halifax, and Mr. Joseph Teale, of

* *Medico-Chirurgical Review*, July, 1872.

Leeds, where the effusion was not only bilateral, but there also existed an effusion into the pericardium. The patient was suffering from acute rheumatism, and had effusion into two-thirds of the left pleural cavity, one-third of the right cavity, and into the pericardium. Death was imminent. They first determined to tap either the left pleura or the pericardium, but finally decided upon the left pleura. Mr. Smith drew off some twenty ounces of highly-fibrinous exudate, with the result of setting up rapid absorption, not only in this cavity, but the other two cavities also, and, as he says, had the unmistakable satisfaction of snatching a fellow-creature from the edge of the grave. Dr. Allbutt adds that, owing probably to the mischance of a small adhesion, the lung was pricked in this case; air welled quickly forth, and the dyspnœa was aggravated for a few minutes to a desperate point; but, as this danger passed off, no other ill consequences appeared. He also stated that in highly-inflammatory cases, such as acute rheumatism, there does not seem to be much risk in turning the fluid into pus; but in the sub-inflammatory, cachectic, and latent forms, in which also tapping is more often required, the tendency to pyoid conditions being stronger, we are more apt to have a tapping for serous effusion followed by empyema.

In a letter from Dr. Henry I. Bowditch to Dr. Clifford Allbutt, published in the *Practitioner*, of November, 1872, with reference to the proper size of trocars, he states that an American "exploring" trocar, simply made thick enough to safely thrust between the ribs, is what he has used ever since he first operated in this way, as suggested by his friend Dr. Wyman; also that an "exploring trocar" and suction-pump are essential points of Dr. Wyman's idea, and hopes thoracentesis (i. e., when used freely) will be always performed with such a trocar, unless in some cases where pus already exists, when he believes we must make long incisions between the ribs, and not trust to large trocars, or drainage-tubes, etc.

In a note Dr. Allbutt states that in England almost any exhausting trocar and canula, with an apparatus to prevent the ingress of air, gets, rightly or wrongly, the name of

"Bowditch trocar;" often, no doubt, wrongly. These vary very much in size, and are often, no doubt, made as small as those used by Dr. Bowditch himself. But they are often much larger, and, he thinks, with the hope of preventing occlusion. He writes that he cannot be sorry to have drawn forth so valuable an expression of opinion from Dr. Bowditch, who is, he states, perhaps the first living authority on the subject.

In a paper read before the Obstetrical Society of London, by Dr. Playfair, on the "Treatment of Empyema in Children,"* the author described peculiarities of pleurisy in children, as contradistinguished from the same disease in the adult. He then referred to the change of opinion which had of late years been observed with regard to the operation of paracentesis. This operation, he says, in ordinary serous pleurisy, evidently stood on a very different footing from the same operation in empyema. In former years we only sought to relieve the distention by removing some of the fluid, and allowing the remainder to be more readily absorbed, while in the latter the chance for absorption was diminished to a minimum; and it would be a great gain if we could effect continuous drainage of the pleural cavity, and at the same time effectually exclude the entrance of air.

The author then described the method of drainage by Chassaignac's tube, with illustrative cases. He also described the method of cutaneous subaqueous drainage advocated by himself, and related the history of three cases successfully treated by it—"the results," he states, "being very satisfactory, and contrasted remarkably with the cases treated by pneumatic aspiration by Bouchut." The paper was illustrated by drawings of the chests of five children, taken by Dr. Gee's cystometer.

Dr. Hilton Fagge felt bound to mention to the Society that he had recently had a case in which the same method had been employed, and with results not entirely so satisfactory. The pus had, in this instance, made its way by the side of the

* London *Lancet*, January 27, 1872.

India-rubber tube, and continued to discharge. It was of great importance that the tube should be tightly grasped by the skin, and for this reason it was better to remove the canula before introducing the India-rubber tube, which should be of the same diameter.

Dr. F. T. Taylor mentioned a case treated in a similar manner.* The chest was tapped with a siphon-trocar. An India-rubber tube was affixed to the canula, and carried into a basin of water. The canula and the tube were retained twenty-four hours, and then a piece of elastic catheter, with a smaller India-rubber tube, was inserted through the canula, which was withdrawn. The tube was retained for fifteen days.

Dr. Sedwick † had, during the last fifteen or sixteen years, carried out the same principle by using a canula, the tube of which projected externally and much beyond the shield, on which he slipped a long India-rubber tube with the other end dipping into a dish of water. The plan of introducing the India-rubber tube into the chest, he thought, was much better than leaving the canula in. Dr. Sedwick had adopted the same plan in a case of paracentesis abdominalis, where the patient was very weak, and the abdominal walls were almost as thin as parchment. The rapidity of the flow was entirely under control by means of pressure on the tube.

In the St. George's Hospital Reports, vol. v., 1870, ‡ Dr. H. W. Fuller states that his advice, founded on large bedside experience, may be summarized thus: 1. Tap whenever dyspnœa is very urgent, or as soon as it becomes evident that remedies fail to produce absorption of the fluid in the chest; 2. Tap as low down as possible, and make a free opening, allowing the chest to empty itself thoroughly; 3. So far as possible, avoid causing any local irritation; 4. If the fluid withdrawn is serous or sero-sanguineous, close the opening with carbolic plaster as soon as the operation is concluded: if, on the contrary, the fluid is purulent, adopt some means to pre-

* London *Lancet*, January 27, 1872.. † Ibid.

‡ "Braithwaite's Retrospect," January, 1872.

vent the wound from closing, and take care that the matter is allowed to drain off as fast as it is formed; 5. After the operation, support the patient by bark and good nourishment, and for a day or two give him opium if necessary.

POINT OF ELECTION FOR PERFORMING THE OPERATION OF THORACENTESIS.

When the opening is to be closed, the lateral aspect of the chest between the seventh and eighth ribs appears to be the most favorable point; I admit that this is not the most dependent portion, but with the instrument used, such as every surgeon has, viz., a catheter, with fenestræ only near the extremity, its point may be passed down near the diaphragm, then the chest can easily be emptied of its liquid contents.*

When a free incision is to be made and allowed to remain open, and where the most dependent part is desired, we might accept the results of the experiments of Freteau, of Nantes, who has done much to settle this question. He says that he performed the operation on the right side between the ninth and tenth ribs, and on the left side between the tenth and eleventh ribs, in more than thirty bodies, and always opened into the thoracic cavity, commencing the incision close to the edge of the latissimus-dorsi muscle, or about three inches and a half from the spine; an operation in this place, Mr. Guthrie states, "should always be performed by incision, and not by trocar." †

Mothers have told me that the first symptom noticed was, the great pain caused when the child would be raised, by placing hands under the arms, necessarily compressing the affected part. Patients with considerable liquid in chest prefer the semi-recumbent position while sleeping. The heart, as a rule, occupies an abnormal position, and the head is usually held leaning toward the affected side, to facilitate passage of air through healthy bronchus. Abscesses may form, whereby

* *See* "Comments on Remarks," by Dr. Leale, in Transactions of New York Academy of Medicine, vol. iv., No. 1.

† Mr. Guthrie's "Lecture on Empyema" (London *Lancet*).

an exit of the pus may take place, but I think that death by suffocation, as a rule, ensues if no outlet by art is made.

There is dullness on percussion, corresponding with the level of fluid contained in the chest; when partially filled this is changed to compare with the height of the fluid in its different positions: in a child two years of age, having about ten ounces of liquid in the chest, there would be complete dullness on percussion up to nipple while in the sitting posture, and over the same space resonance anteriorly while lying down on back; and it is by this method that we are enabled to diagnosticate the quantity of fluid before an operation has been decided on. In the case of N. B., where air and pus mingled freely, the splashing sound was easily produced by shaking the patient.

The circumference of the chest is perceptibly increased, especially in children—the exact difference can easily be noted by taking the middle of the sternum and spinous processes as guides, passing directly over the nipple, where, as a rule, the bulging is more marked; of course care should be taken to get measurement of normal side, both before and after inspiration. The accumulation may be to such an extent as to disarticulate the clavicle, as in M. H. The affected side during the respiratory act is almost if not altogether motionless, while there is increased expansion on the well side, and severe pain during inspiration is a frequently-occurring symptom.

Method of operating, after having been satisfied by an Exploring Operation done painlessly by Ether-spray.—A half-inch incision is made by firmly holding a sharp scalpel and directly cutting down upon the eighth rib to the bone, then drawing up the integument, still holding the knife *in situ* until the incision covers the space between the seventh and eighth ribs, beneath angle of scapula, when it is pushed forward (which in nearly every instance would be less than half an inch), when it enters the thoracic cavity. The opening is sufficiently enlarged to admit the point of a No. 8 male silver catheter, which may be gently pushed downward until it is near the diaphragm; the chest may be easily emptied of any

fluid by gentle and continuous suction, as before noticed in the case operated on by Prof. Flint, in 1865. Sometimes, during the withdrawal of the fluid, slight attacks of syncope and occasional dyspnœa occur, but they usually are of short duration, and generally cease if the suction is discontinued for a few moments. After the liquid has been withdrawn, the catheter may be slowly removed, and as its edge is freed the integument can be easily slid back to its original position, directly over the eighth rib. This opening, which is now valvular in character, should, if desired to be closed, have its edges held in apposition by means of two sutures, the surface then dried and covered by small strips of adhesive plaster, and over the whole a moderately firm roller applied, which will prevent emphysema of the surrounding cellular tissue. As a rule, the wound will be entirely healed at the end of twenty-four hours, when the stitches should be removed, and, for additional safety, the compress kept applied for three or four days.

But if the wound is desired to be left open, then an incision at least two inches in length should be made, to permit of free drainage.

During the operation we can decide whether the opening should be closed or left open: I do not think that this should be decided by adopting the general rule, viz., where there is pus, leave a free exit, and where there is serum close the opening, but, from past experience, should say when the pus is what the older surgeons termed laudable, and especially when, by its density, etc., we suppose that it has commenced to undergo the usual change preparatory to absorption, then I think that recovery will be much hastened by closing the wound, and allowing what remains of pus or air to be absorbed, as noted in several of the cases before cited.

But if the fluid have any offensive smell, then not only should a free opening be allowed to remain, but the chest should be thoroughly washed out occasionally with the solution so successfully used by Prof. Peaslee after ovariotomy, viz., a mild solution of chloride of sodium and carbolic acid.

This, however, should be discontinued as soon as practicable, when the wound can easily be closed, which causes far less debility, and a more speedy restoration of the lung to its appropriate condition. There are yet physicians who contend that it is better to allow the fluid to remain in the thorax than resort to any surgical interference. In answer to such, we can say that the danger is not only that by continuance of the constantly-increasing exhaustion, but death sometimes happens suddenly, when least expected, as occurred to the brother of a governor of a neighboring State, who, while in Albany, retired in ordinary health, and was found dead in his bed on the following morning, the pus having suddenly perforated the bronchi, and, before assistance arrived, life was extinct.

With reference to the admission of atmospheric air into the chest, I am also convinced by experience that this always follows to a certain extent where a large quantity of fluid is withdrawn. But then it has been proved that air, blood, and pus, can be hermetically sealed up in the chest, and be absorbed. In the case of Conaughton, there was a large quantity of air and blood closed up in the chest, yet in a very short time all had disappeared. Also in the instance of Captain N. B., atmospheric air, blood, and pus, were confined in the chest, and, as recorded, disappeared, and four years afterward he was in excellent health. Mr. Guthrie mentions a case where the admission of air was so great that, to relieve distressing symptoms, it was removed by a syringe.*

"Why should the trocar be retained for thoracentesis, any more than for tracheotomy?" asks Trousseau.

In 1586, Sanctorius, who seems to have been the first to practise bronchiotomy, proposed puncture of the trachea with the trocar, which he had invented for abdominal paracentesis.† If we wish either to avoid injury to other organs by puncture, or the admission of air into the cavity, we think the scalpel possesses advantages over any instrument which is forcibly

* Guthrie on Empyema (London *Lancet*).

† Trousseau, "Clinical Medicine," vol. ii., English translation.

thrust sometimes into vital organs, which would not be the case when we see tissue by tissue through which we cut.

Dr. Watson relates a case seen by him, where death was caused by using the trocar: he states that the integuments of the side were œdematous; and it was thought that a little serum issued upon the passage of the grooved needle. The serum must have come from the infiltrated areolar tissue. No liquid was evacuated by the trocar. The patient died a day or two afterward, of peritonitis. The instrument had perforated the diaphragm, and entered the spleen, which was unusually large. *

The skillful Laennec also had an unfavorable case, resulting from the use of the trocar, where the operation was performed at his favorite seat, viz., between the fifth and sixth ribs; he thrust the instrument, as he supposed, into the thorax, and was a good deal surprised to find that no gush of liquid followed its introduction. The patient died, and dissection showed that the trocar had entered the cavity of the abdomen after transfixing the diaphragm, which, having been forced upward by a large lever, had contracted firm adhesions to the seventh rib.†

Finally, I think that I can safely say, after carefully considering the subsequent histories of all the patients on whom I have performed thoracentesis during the past eight years, that in every instance it has proved successful, either by not only preventing death, preceded by the most agonizing symptoms that a physician is called upon to witness, but in the majority of instances we may confidently hope a restoration to at least very good health.

I should, as in tracheotomy and abdominal paracentesis, prefer to use the scalpel to open the chest: 1. As a safer procedure; 2. An incised wound is known to heal (if required) with greater certainty; 3. That, by using a long male silver catheter, the most dependent part of the chest can be emptied of its fluid contents, and there is no danger of pricking the

* Watson's "Practice of Medicine," London, 1857, p. 141.
† Ibid.

lung from change of position or movement of patient while the liquid is being withdrawn, as noted by Dr. Allbutt.

4. That when pus has commenced to undergo that change preparatory to absorption, the probabilities are, that very little, if any, will be reproduced after the operation, if the wound is immediately closed.

5. That, in closing the wound under the above circumstances, the little atmospheric air admitted, and the small quantity of pus left behind, are very soon absorbed.

6. That if pus should again accumulate in the chest, the operation is so easy, the pain so slight, and the closure so rapidly accomplished, that a repetition is nothing to be feared, and really causes less prostration than where a large incision is made, and possibly pus formed with greater rapidity.

7. That atmospheric air, pus, and blood, even to the extent of about eight ounces, may be absorbed, and that the injured compressed lung can again resume its normal condition, as so conclusively proved by the recorded *post-mortem* examination.

8. That, when unhealthy decomposition has commenced, the wounds should be left open, and the parts carefully disinfected.

9. That thoracentesis should oftener be performed for the quick removal of fluid from the chest, even as recorded during far advanced phthisis pulmonalis, when relief may be obtained, life prolonged, and painful death averted.

Dr. Peaslee remarked that thoracentecis was a subject of great practical importance, and has been developed more in this country than anywhere else. He had performed the operation many years ago, before Dr. Bowditch did. In 1851, visited a patient, a man twenty-four years old, who had hydrothorax and ascitis abdominis, which came on two months before. He drew off seventeen or eighteen pounds of fluid from the peritoneal cavity, and then eighty-six ounces from the thoracic cavity; used an ordinary trocar, made the puncture between the sixth and seventh ribs, and not quite so far back as Dr. Leale had in his cases.

Dr. Peaslee considers it safe to make the opening anywhere in the convex intercostal spaces where there is fluid, taking care not to wound the pericardium. His plan was to draw the skin over the rib downward, so as to puncture it in middle of the intercostal space; when the instrument is withdrawn, the skin will slide up again and close the opening.

On withdrawing the trocar, he introduced a gum-elastic catheter, the patient leaning to that side, so as to prevent the entrance of air into the thoracic cavity.

Stated Meeting, March 20, 1873. Dr. Austin Flint, President, in the Chair.

After the transaction of usual business, the following paper was read, entitled:

FACIAL PARALYSIS TREATED BY A NEW METHOD.

By WILLIAM DETMOLD, M. D.

Every practitioner, I presume, is familiar with facial paralysis. I do not now allude to that paralysis which follows an apoplectic attack, but to that local affection—paralysis of one side of the face—which comes on suddenly, at times, somewhat analogous to infantile paralysis of the lower extremities, in consequence of a draught of cold air, at other times without any appreciable cause, and which generally yields to various kinds of treatment; and, in most cases, probably would get well without any treatment in from four to six weeks. But occasionally we encounter cases which assume a chronic character, and, defying the ordinary methods of treatment, lead to a permanent distortion of the face.

I reported, some years ago, to this Academy, some cases of facial paralysis in which I successfully performed myotomy. I only reported these cases orally, and I believe they never have been put on record; but, as they form an interesting contrast to the case which is the subject of this paper, I will briefly record them here:

Case I.—Miss H., about twenty-one years of age, had, in early infancy, paralysis of the face, which resisted every effort of treatment; for, as the family were in very affluent circumstances, it is to be presumed that the case had not been neglected. When I was consulted, I found, on one side of the neck and below the ear, a number of deep, adhering scars from scrofulous ulcerations, which possibly may involve some branches of the facial nerve. Whether they were the cause of the paralysis I do not know, for, as the mother of the young lady had died years before, I could not obtain a satisfactory history of the case. The prominent feature of the affection was a strong contraction of the muscles of the other side, which drew the mouth considerably over to that side. As I had reason to suppose that the ordinary methods of treatment had been exhausted, evidently without effect, I did not deem it worth while to go over the same ground again, but I determined at once to divide the contracted muscles. I made, on the inside of the cheek, a semicircular incision through the mucous membrane, dividing every thing, till I felt that I had reached the cutis. The incision commenced near the ala of the nose, and was carried around the fibres of the orbicularis oris to near the middle of the lower lip, thus dividing the insertion of all the muscles that attach themselves to the orbicularis on that side. There was some bleeding, which, however, yielded to pressure, and I had the satisfaction of relieving the deformity almost entirely—at any rate so far that what remained appeared more like a trick or bad habit than a deformity. When the face was at rest, nothing was apparent; only when the muscles were in action, especially in laughing, there was still a contraction visible, and even that, I think, might have been cured by repeating the operation, but the young lady was so well satisfied with the result obtained that she would not consent to a second operation.

Case II.—Although this case does not strictly come under the name of facial paralysis, yet, as the treatment was analogous, its brief report here may not be out of place:

Sergeant B., during the Mexican War, received at the bat-

tle of Cerro Gordo a gunshot-wound, the ball entering the mouth, carrying away a portion of the hard palate, breaking the upper part of the ramus of the lower jaw, and making its exit below and behind the lobe of the ear, probably dividing the facial nerve. I saw him several years afterward. There was considerable deformity, besides anchylosis of the lower jaw, but the object of his consulting me was the eye of that side. The orbicularis palpebrarum seemed entirely paralyzed; the upper lid was forcibly drawn up, so that the eye could not even be partially protected by the forcible rolling upward of the ball as we see it in the ordinary cases of paralysis, where the lids cannot be completely closed. The cornea, in consequence of the constant exposure, had become vascular and opaque, and the constant irritation from that source induced the man to apply to me, upon the advice of his physician, for the purpose of having the eyeball removed. Before proceeding, however, to that extremity, I determined to give him the benefit of myotomy. I made a semilunar incision below the supraorbital ridge, and divided the levator palpebræ superioris before its fan-like insertion into the tarsus. The lid dropped immediately, and, even before the external wound was healed, followed the motion of the lid of the other eye. In a few months the cornea lost its vascularity and opacity, and there was scarcely a trace of the previous paralysis of the lid remaining.

In both the foregoing cases, the prominent feature was contraction of the non-paralyzed muscles. I therefore tried, by dividing them, to put them more nearly on a par with their paralyzed antagonists, and in both cases success justified the attempt.

The case which is the proper subject of this paper is of an entirely different character, there being hardly any muscular contraction, and therefore an entirely different mode of treatment became necessary. That is the reason why I have placed these cases here in juxtaposition.

Miss N., now about eighteen years old, was seized, when about two years old, with one-sided paralysis of the face. A

number of physicians have attended the case from time to time and in succession, but without result. She tells me that I myself was consulted years ago in the case, but that I have not done more or better than the rest. During a recent visit to Europe the father of the young lady was advised to apply to me, and thus I was again consulted. The patient now presents a very marked case of paralysis, the main feature of which is not contraction of the other side, but, in consequence of complete inaction of the zygomatic muscles and the levator anguli oris, a heavy drooping and hanging down of the angle of the mouth. Knowing that the ordinary methods of treatment, such as stimulating frictions, hot douches, endermatic use of strychnia, electricity, etc., etc., had been tried conscientiously and without effect, I determined to try what mechanical means would do. I bent a wire into a hook, which I put into the drooping corner of the mouth, and, drawing it up, bent the wire over and behind the ear. I recommended the patient to keep it on overnight, trusting that, by entirely relaxing the paralyzed muscles, and supporting the dragging weight, I might somewhat relieve the defect. She reported herself next morning, full of joy. The result exceeded my most sanguine expectations. After one night's use of the wire, the drooping of the mouth had diminished in a very marked degree, but the wire had cut into the corner of the mouth and made it sore. I therefore ordered an instrument to be made of silver by Otto & Reynders, which should obviate the difficulty. It consists of a flat hook, with the edges turned out, and terminating in a wire hook, which goes over the ear. She wears this instrument steadily at night, only omitting it when the corner of the mouth gets sore; and she is steadily improving.

It then occurred to me that I might make this instrument still more effective if I could combine with it a permanent and continuous galvanic current through the paralyzed parts by having it made of two different metals, thus forming as it were a single cell of a galvanic battery. With this view I had the flat hook which enters the corner of the mouth made

of platina, and the wire terminating in a plate behind the ear, made of zinc. Mr. Charles T. Chester, who was kind enough to make this instrument, gives in a note to me the following account: "I charged the zinc plate with salt and water. I have no exact instruments to measure quantity of current passing, but it holds my galvanometer at ten degrees deflection through the resistance of nine hundred British Association units. A steady current of appreciable power constantly flows through the part when the velvet (which covers the zinc plate) is moistened."

I am fully aware that the mode of application is somewhat opposed to the generally-accepted theory that the galvanic current which runs in the direction from the hard metal to the softer should correspond with the direction of the current of the nerve-fluid, that is, from the centre to the periphery. But, on the one hand, I do not consider the force of this theory sufficiently demonstrated, and, on the other hand, I did not want to put the softer and easily-oxidized metal into the mouth. Consequently, when my instrument is applied, the galvanic current, instead of running with the nerve-current, runs opposite to it, but, whatever the direction is, the galvanic current runs exactly through the affected and paralyzed parts.

I must not omit here to remark that, during the time the galvanic instrument was being made, I had given the silver instrument as a model, and that consequently for about a week no instrument was worn by the patient. At the end of the week a considerable relapse of the paralysis was noticeable, showing the necessity of a long-continued use of the instrument.

The galvanic instrument has now been worn for a few weeks, and the patient is steadily improving; but, as the recovery had already far progressed and was steadily progressing, before galvanism was brought into coöperation, I am unable to say what share in the benefit, or whether any, is due to the galvanic current, to which, on the whole, I do not attach as much importance as to the mechanical support. The application, under the circumstances, I believe and claim as new.

I have thus briefly put representative cases of two classes of facial paralysis together: one where the contraction of the non-paralyzed muscles forms the prominent feature, and the other where the inaction of the paralyzed muscles is prominent; and I have shown two entirely different methods of treatment of the two classes; but I have no doubt that many cases may occur where both methods of treatment might be advantageously combined.

Dr. J. C. DALTON then read a paper entitled "Galen and Paracelsus." (Published elsewhere.)

The Academy then adjourned.

STATED MEETING, APRIL 3, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

DRS. LEROY M. YALE, JOHN J. MASON, and CORNELIUS R. BOGERT, were elected Resident Fellows. Dr. W. J. SLOAN, Surgeon U. S. Army, was elected a Non-Resident Fellow.

The PRESIDENT introduced Prof. J. P. WHITE, of Buffalo.

Dr. ROBERTS announced the death of Dr. JOSIAH C. NOTT, aged sixty-nine years, at Mobile, Ala., on March 31, 1873.

The PRESIDENT then announced the paper for the evening.

ON THE HISTORY AND TREATMENT OF ACUTE URÆMIA.

BY ALFRED L. LOOMIS, M. D.,

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MR. PRESIDENT, at your request, and in compliance with your suggestion, I will, this evening, make a few remarks on the history and treatment of *Acute Uræmia.*

Under this term may be grouped two classes of symptoms, which differ in their mode of development and in their attendant phenomena.

In the one, nausea, vomiting, and headache, usher in twitchings and epileptiform convulsions of the voluntary muscles, a state which has received the name of uræmic convulsions.

In the other, headache and drowsiness, or convulsions, usher in a state of insensibility, which has received the name of uræmic coma.

The primary cause of both these conditions is always to be found in a failure of the kidneys to perform their normal function of elimination, and the consequent accumulation in the circulation of some or all of the poisonous elements of the urine. This condition may occur in the course of any disease in which suppression of the renal secretion takes place. Such arrest of the function of the kidneys most frequently occurs in scarlatina, in the different forms and stages of Bright's disease, in the puerperal state, and in connection with the surgery of the urethra.

Whatever the poisonous agent may be, it unquestionably acts directly upon the cerebro-spinal centres. In its action, it does not seem so much to directly excite convulsions or coma, as to increase the irritability of the nervous system, and the consequent liability to convulsions from causes which, under other circumstances, would produce no noticeable disturbance.

A number of theories have been advanced in regard to the exact element which acts as the poisonous agent.

The earliest accepted view is that which attributes the symptoms of uræmia to retained urea. This view, originally proposed by Willis, has been ably sustained by the experiments of Drs. Richardson and Hammond; and, although at different times it has been discarded and apparently disproved by the experiments of distinguished observers, to-day it is the accepted view of most authorities.

Some years since, Frerichs made the statement that urea as urea is innocuous, and advanced the theory that the poisonous agent was carbonate of ammonia resulting from the decomposition in the blood of urea into carbonate of ammonia and water, which decomposition he ascribed to the action of a ferment in the blood. This theory has been entirely overthrown by other experimenters.

Still more recently, Traube has advanced an hypothesis which has attracted much attention (Stewart). "He points

out, that, as in Bright's disease, the blood-serum being in an impoverished state, tends to transude, and, in consequence of hypertrophy of the heart, the blood-pressure in the arterial system is increased; so, when from any cause this blood-pressure is suddenly increased, or the density of the blood-serum is further diminished, serous fluid transudes through the small arteries, and œdema of the brain results. The result of this is, that the capillaries and veins are compressed, and the brain becomes correspondingly anæmic. The form of the uræmic attack varies according to the part of the brain which is so affected. If the cerebrum alone is involved, coma appears; if the pons Varolii and medulla oblongata alone, convulsions; if both together be affected, the result is combination of coma with convulsions. This hypothesis is certainly well worthy of being carefully investigated, for the condition of the brain met with in fatal cases of uræmia often accords with it, at least in the chronic cases in which death occurs from uræmia. But I have some difficulty in accepting it as explaining uræmia in acute cases. It is to be remembered that its author does not claim for it any position higher than a mere hypothesis."

The experiments of Oppler and others go to show that urea is formed by the kidneys from the nitrogenous materials in the blood, and that the uræmic manifestation depends mainly upon the accumulation in the blood of creatin and creatinin. Oppler also found that there is a retention of muscle-waste in cases of uræmia, and conceived that there may be a similar retention of the products of the nerve-waste; and to the deleterious influence of this substance he would ascribe the symptoms.

Dr. Grainger Stewart makes the statement that some forms of uræmia *may* be associated with structural changes in the brain, similar to those which occur in the retina in cases of neuro-retinitis.

The experiments and facts upon which these different theories are based lead to the following conclusions:

1. That uræmic toxæmia, acute and chronic, depends on a complete or partial arrest of the urinary secretion.

2. A qualitative analysis of the constituents of the urine goes to show that urea is its only positive poisonous ingredient, and "that *it* is not the special product of any one particular tissue or organ, but the united product of all nitrogenized effete matter."

3. Numerous experiments show that urea, when introduced into the blood of animals, acts as a narcotic poison, producing phenomena identical with those of uræmia.

4. That urea is not decomposed into carbonate of ammonia and water in the blood, but that such decomposition may take place outside the blood-vessels in the bladder, pelvis of the kidney, and intestines; and if the products of the decomposition are retained in these cavities any length of time, they give rise to a condition of ammonæmia, which in many of its features resembles uræmia.

While, therefore, the question as to the exact poisonous agent in uræmia is still unsettled, it seems to me that the facts relative to urea warrant the assumption that urea is an irritant poison, and when in excess in the circulation acts primarily upon the cerebro-spinal centres, and through them interferes more or less with the functions of organic life; and that œdema of the brain and other structural changes which occur in the course of uræmia are the results of the action of this poison.

An acute uræmic attack is usually preceded by certain premonitory signs, such as œdema in various parts of the body, restlessness, or an almost irresistible desire to sleep, vertigo, headache, delirium, nausea, vomiting, and impaired vision; the countenance has a pale, waxy, or dingy appearance; the urine is scanty, high-colored, bloody, albuminous, and contains casts. After the appearance of the premonitory symptoms the progress of the mischief will vary in different cases according to the amount and cause of the retention of the urea.

Thus if a large amount of urea is suddenly thrown into the circulation and retained by a continuance of the arrested elimination, or increased by a continuance of the producing

cause, the body and extremities become violently convulsed, or the patient passes rapidly into a state of coma.

The convulsion may consist of a single paroxysm, or a succession of paroxysms may follow one another at intervals of a few minutes or several hours, the patient lying during the interval in a state of more or less profound insensibility. During the convulsion the face becomes livid, eyes glassy, the pupils contracted or dilated. At the commencement of the convulsive attack they are generally contracted; frothy mucus, which is sometimes bloody, collects around the mouth, and there is a strong urinous odor emanating from the perspiration. The pulse is accelerated, and the temperature is raised in some instances as high as 107°.

Uræmic coma may come on gradually, twenty-four or forty-eight hours elapsing before the stupor is complete, or the patient may fall suddenly into a state of profound coma, its advent resembling an attack of cerebral apoplexy. There are periods when the coma is so profound that nothing arouses the patient; at other times he is easily aroused, or arouses himself, and attempts to speak and to sit up, swallowing fluids with little difficulty.

When urea is gradually introduced into the circulation, or is freely eliminated, as in cases where renal disease is slowly developed, the system becomes accustomed to the presence of the poison, and thus a considerable excess of urea may exist in the blood for a long period without giving rise to any but the premonitory symptoms of acute uræmia; but, when once the balance is destroyed and a certain excess of urea in the blood is reached, the kidneys become embarrassed by the excessive demand made upon their excreting power, and rapid and intense renal congestion follows, the nerve-centres are overwhelmed, and either convulsions or coma, or both, follow, and thus acute uræmia may be developed in the chronic as well as in the acute stage of renal disease.

Uræmic coma is always accompanied by a certain amount of stertor; the respirations are accelerated at first, but they soon become slow and labored. The pupils are dilated, but

they are not irregular; the pulse is more rapid than natural, and lacks firmness. The temperature at first is raised, but after a time falls below the normal standard. Acute uræmia simulates in some particulars so many diseases in which convulsions and coma are the leading symptoms, that it is difficult to give directions which shall enable one to always separate it from analogous disorders. I will name a few of the more prominent points in its differential diagnosis.

The phenomena of an epileptic seizure are almost identical with those of uræmia, and in some instances the task of distinguishing the one from the other would be exceedingly difficult unless the previous history be admitted.

If the patient's history is known, the chronic character of the epilepsy is sufficient to distinguish it from acute uræmia, and an examination of the urine positively determines the uræmic character of the convulsion. At the time of the paroxysm a distinction may also be drawn, for in epilepsy one side is convulsed more violently than the other, while in uræmia both sides of the body are equally affected by the convulsive movements. In epilepsy, although there is loss of consciousness, reflex sensibility continues from the beginning to the end of the paroxysm, which is not the case in uræmia. Immediately following uræmic paroxysms there is deep coma; following an epileptic seizure there is merely a deep sleep, from which the patient may be aroused.

In cerebral apoplexy, coma always precedes convulsions, and with the convulsions there are facial paralysis and hemiplegia; there is also clonic spasm of the paralyzed parts, and the urinary symptoms of uræmia are absent. In hysterical convulsions the patient falls into a convulsive, tetanic, or cataleptic condition, with a scream. Close inspection shows that the patient is not unconscious, and the pupils are normal, as are also the pulse and temperature. The limbs are jerked irregularly, the breathing is jerking and spasmodic, and is attended with a choking sensation. There is no lividity of the face or distention of the cervical blood-vessels, and the close of the paroxysm is usually accompanied by the discharge of a large quantity of pale urine.

Cholemic convulsions, or those that occur when the blood is overcharged with the constituents of the bile, in their phenomena very closely resemble uræmia, but may be distinguished from them by the jaundice which precedes or accompanies their development, and by the antecedent history of acute hepatic affections. Convulsions originating in meningitis and other cerebral affections are distinguished by the accompanying characteristic symptoms of these affections.

The main points in the differential diagnosis of uræmic coma are identical with those of uræmic convulsions. It may be distinguished from the coma of apoplexy by the absence of paralysis, from opium-poisoning by the rise in temperature (the temperature in uræmic coma being generally above 100°, while in the coma from opium it is below the normal). The slow and peculiar character of the respiration in opium-coma also distinguishes it from uræmia.

The condition of the pupils is not a safe guide, for not unfrequently in uræmia they are as contracted as in opium-poisoning.

It is distinguished from epileptic coma by the antecedent history, and from rum-coma by the alcoholic surroundings of alcoholismus.

In all cases of coma, an examination of the urine is necessary to complete the diagnosis.

Causes of Death in Acute Uræmia.—Apparently the primary cause of death in uræmia is the accumulation of urea in the circulation, which acts as a true narcotic poison, resembling in its *modus operandi* other narcotics, of which belladonna and opium are the best types. When introduced in so small quantities that its elimination can be accomplished in a short time, it produces a moderate sleep; but, when the quantity is sufficiently large to overtax the eliminating powers, it causes death by arresting oxidization.

Dr. Watson remarks that, whatever may be the nature of the unknown and perhaps fugitive condition of the nervous centres in uræmia, which is capable of arresting or abolishing their function, it is important to keep in mind a distinct and

clear conception of the fact that there must be some such physical condition.

Dr. Richardson claims that there is first a direct toxical effect of the urea, acting by depression on the muscular and nervous system, and that in acute uræmia, as a result, the blood undergoes physical modifications which render it incapable of supporting the changes which constitute natural life. These modified states of the blood, consisting of increase of water, diminution of red corpuscles, modifications in the physical construction of the remaining cells, and accumulation in the mass of blood of a true toxic agent, tend to render that fluid incapable of undergoing normal chemical reconstruction in the pulmonic current. Such blood is incapable of combining with the oxygen of the air; the arterial blood consequently loses its active colorific life-sustaining properties, and the final result is secured by what may be most properly designated apnœa commencing in the circulation.

Dr. Moreland states that acute uræmia, marked by the occurrence of convulsions and coma, leaves the brain anæmic in appearance, and probably somewhat softened.

From the many facts, experiments, and statements made by competent observers, as well as by the clinical history of uræmia, it is evident that the primary cause of death is a narcotic poison, the exact nature and action of which we do not understand; that the primary action of this poison is on the nerve-centres, producing certain changes in the blood, which interfere with or arrest oxygenation; and this is followed by certain structural changes taking place in the different tissues of the body, which make up the *post-mortem* history of the disease.

I have now come to the special object of this paper, viz., to the consideration of the treatment of acute uræmia.

Treatment.—I shall first give a brief synopsis of the most prominent views of the present day—the views of standard authorities. All agree in this, that in the treatment of acute uræmia, to secure as rapidly as possible a free eliminative action, either by the skin or bowels, or by both, or by the kidneys, is of the first importance.

Frerichs is the only authority that proposes to neutralize the uræmic poison, which he claims is ammonia. To accomplish this, he directs the inhalation of chlorine gas, or the internal administration of the vegetable acids.

With most authorities, the favorite method is diaphoresis, accomplished by vapor or hot air. It is claimed that, by a vicarious action of the skin, the excrementitious products which normally fall to the lot of the kidneys to excrete, are removed from the system in the cutaneous perspiration.

In connection with the process of elimination, a vicarious action of the bowels is induced by the internal administration of drastic purgatives; elaterium and scammony are the favorites to accomplish this hydragogue catharsis; and it is also claimed that by this method the alimentary canal eliminates the products which should normally find their way out of the body by the urine.

The testimony of authors on the utility of diuretics in the treatment of acute uræmia is conflicting.

Dr. Roberts says that his experience has not given him a high opinion of their accuracy.

Dr. Harley says that it ought never to be forgotten that, in acute Bright's disease, as well as in the first stage of all inflammatory and congestive attacks occurring in the course of chronic kidney-affections, diuretics are inadmissible; and he adds that the reason why the employment of diuretics often does harm in acute kidney-affections is readily understood, when we recollect that they have always the tendency rather to increase than diminish the flow of blood to the already inflamed organs.

Dr. George M. Johnston states that, with our present knowledge of renal pathology, it is clear that the practice of giving diuretics in acute nephritis is most unjustifiable.

Dr. Richardson says that one of the greatest errors common to the inexperienced is to give diuretics to a badly-working kidney—an error as unphilosophical as it is unpardonable, for the cessation of the secreting function of the kidney indicates a tendency to congestion of the renal organs. To relieve

renal congestion, not the kidneys, but some other emunctory channels, must be freely opened, and the kidneys left to do as little labor as possible.

On the other hand, Niemeyer maintains that, whatever theoretical objection against the employment of diuretics there may be, in desperate cases recourse should always be had to them.

Dr. Stewart recommends diuretics in the acute stage of the inflammatory form of Bright's disease, to remove the effete material from the uriniferous tubes.

The ground on which diuretics are objected to is, that it is contrary to the principles of medicine to stimulate an inflamed part—that the first step toward the healing of an inflamed organ is rest.

Admitting that this view is sound, we have a class of diuretics that do not in any sense act as stimulants to the kidney. Digitalis ranks first in this list; although a very efficient diuretic, it never seems to irritate the kidneys. The *modus operandi* of this remedy is now well settled (Stewart). By increasing the power of the heart's action, and perhaps, also, contracting the capillaries, it materially increases the blood-pressure. As the normal secretion of urine depends upon that pressure being in a healthy state unopposed by any obstruction, and the diminished flow in this disease is due to obstruction within the tubules, the digitalis appears to supply such an increase of pressure as overcomes the obstruction, and, indeed, carries it away by the force of the current it originates.

Accepting this view of the diuretic action of digitalis, its administration is especially indicated in acute uræmia. To obtain its effects in the condition of the kidneys that attends acute uræmia, I am convinced that much larger doses are required than usually are administered. My rule of practice in these cases is to give half an ounce of the infusion of the English leaves every three hours, for twenty-four hours—or, at least, until I produce the specific effect of the drug—and I do not remember in a single instance to have met with the overwhelming accumulative effects of digitalis of which so many writers warn us.

The experience of every one, I think, will sustain me in the statement that, when acute uræmia is fully developed, and the patient is in convulsions or coma, often (in the majority of cases) the skin and the bowels, as well as the kidneys, lose their excretory action, diaphoresis cannot be induced, or, if induced, is not eliminative, and the bowels do not respond to purgatives, although the patient may swallow them in large doses.

Under these circumstances, Dr. Richardson says that he is "convinced that in cases of acute uræmia there is one and only one remedy to be adopted; and that remedy is none other than the free abstraction of blood."

On physiological grounds, venesection, in extreme examples of uræmia, comes forward as a natural and effective remedy; for, as there is a soluble poison in the blood, we secure in bloodletting the readiest means by which to remove the poison directly. He adds, there is yet another advantage in bloodletting: by it we relieve congestion of the visceral organs, and especially those of the kidneys; hence, it usually obtains that, after the removal of blood, secretion takes place readily, and a response is offered to diaphoretic and purgative remedies which did not before present itself.

Dr. Harley (in his recent work on the urine and its derangements) makes the statement that in some cases of acute uræmia, especially in uræmic eclampsia, venesection may sometimes be had recourse to with great advantage, but its indiscriminate use he regards as highly unphilosophical with our present knowledge of uræmic convulsions.

Dr. Braun, in his monograph on uræmic eclampsia, states that, since the days of Dewees, Burns, and Hamilton, it has been, and still is the custom to find the only power against uræmic eclampsia in abundant general bloodletting, often repeated—a proceeding which he believes can be justified as little by the present state of our theoretical knowledge in regard to this disease as it is by the mortality which follows its employment.

By bleeding, the hydræmia is increased, as well as the dan-

ger of puerperal thrombosis and pyæmia, and not unfrequently the paroxysms are aggravated. A very strong argument against venesection in acute uræmia is the fact that, after extensive trial by the profession, the practice has fallen into disuse.

The question then comes to us, If overwhelming the system by the uræmic poison (marked by convulsions and coma) shuts off for a time all avenues of elimination, what means have we to counteract the effects of this poison, and open again the avenues of its elimination, or, at least, to hold the patient until the normal eliminating process shall be reëstablished?

Our first efforts must be directed to diminish reflex sensibility, and subdue spasmodic muscular paroxysms, for these, if continued, either will directly terminate life, or end in an equally fatal insensibility.

The remedy which for some years has been employed for the accomplishment of this object is chloroform. It has been extensively used, and is, I believe, regarded as the readiest and safest means for controlling uræmic convulsions.

Dr. Braun says, in regard to it, that chloroform narcotism should be induced instantly, when indications of an impending paroxysm show themselves, but that the administration of chloroform must not be kept up during the convulsive attack, or the state of coma.

Dr. Roberts says that, during the convulsive paroxysm, chloroform inhalation is the most prompt and ready means of controlling the spasm; when, however, uræmic paroxysms begin with drowsiness, and gradually pass on to insensibility, or when convulsions occur as breaks, in a continuously comatose condition, chloroform affords no prospect of relief.

Dr. Harley, although he recommends some form of anæsthetic in uræmic eclampsia, makes the statement that, if medical men were more intimately acquainted with the powerful chemical changes which chloroform exerts on the constituents of the blood, even when taken into the system by the lungs, they would probably employ it with more reserve than at present they do.

Sir James Simpson says that, although the direct action of chloroform upon uræmia is doubtful, yet it is certain that, in eclampsia, chloroform is the best palliative, inasmuch as it moderates the paroxysms.

Although many authorities recommend the use of chloroform in uræmic eclampsia, few make mention of its employment in acute uræmia, independent of the puerperal state. Its only known clinical effect is to control muscular spasm, and in a large proportion of cases it fails to give more than temporary relief to those patients who pass from one convulsion to another into a state of complete coma, and die without any apparent neutralizing or eliminating effect from the chloroform.

In the few cases in which I have administered chloroform in non-puerperal uræmic convulsions, it has seemed to me to have no other effect than to arrest convulsive movements, by rapidly hastening my patient into a state of insensibility. In no instance have I known its administration to be immediately followed by diaphoresis, or a return of the urinary secretion; and it has seemed to me to be more difficult to establish diaphoresis or diuresis by diaphoretics or diuretics in patients with uræmia to whom chloroform has been administered, than in those who had not taken it. I believe, therefore, that while it temporarily controls muscular spasm, it prejudices the chances of ultimate recovery, by the changes its inhalation produces in the blood, which changes increase rather than retard the uræmic toxæmia.

With these impressions, one naturally seeks an agent that not only has power to control muscular spasm, but at the same time by its action shall tend to reopen the avenues of elimination, either by counteracting the effects of the uræmic poison on the nerve-centres, and thus facilitate the action of diuretics and diaphoretics, or itself act directly as an eliminator.

I believe morphine, administered hypodermically, to be such an agent.

This brings me to the question which has led me to this discussion this evening, viz.:

1. Can morphine, in full doses, be hypodermically administered to patients in acute uræmia without danger?

2. What are the effects which follow such administration?

If we turn to acknowledged authorities for an answer to the first of these inquiries, we find that nearly all make mention of opium only to warn us of the danger attending its administration. I will quote from a few of them: Dr. Harley states that "although Dover's powder may be given with impunity, opium can seldom be employed in kidney-affections in any other form without a certain amount of risk. More than one example of its deleterious effects in such cases has come under my notice. In cases where there is a tendency to convulsion, even Dover's powder must be cautiously used."

Dr. George Johnston states that Dover's powder may be given in Bright's disease, "when the bowels are freely open, the urine not scanty, and there is no headache or drowsiness. In other cases, opium, in any form, would probably be injurious, on account of its tendency to check secretion, and aggravate the symptoms of cerebral oppression. . . . In no circumstances is an opiate so likely to produce dangerous and unmanageable stupor, as when its influence is added to that of urea in the blood."

Dr. George T. Elliot, in a paper on albuminuria in pregnancy, states that in uræmic eclampsia he has always used narcotics very sparingly—codea and McMunn's elixir of opium being his choice. He had resorted to an hypodermic injection of morphine.

Dr. Alonzo Clark, in the most positive terms, warns against the use of opium in uræmia.

Dr. Austin Flint, in his "Practice of Medicine," states that opium should be given with circumspection, and adds that observation has shown that, in moderate doses, it is liable to produce marked and even fatal narcotism, if the blood is surcharged with urea. In the same connection, he alludes to the case of an opium-eater, with Bright's disease, who took a bottle of McMunn's elixir daily, without any apparent ill effect.

It is hardly necessary to multiply quotations to show that the profession has almost unanimously lifted its voice in warning against the use of opium, either in acute or chronic uræmia.

During the first years of my professional life, I regarded opium as one of the most dangerous remedial agents that could be administered to uræmic patients, rarely daring to give more than five grains of Dover's powder to a patient with albuminous urine, and, if convulsions and fatal coma happened to follow such administration, more than once do I remember to have felt that a Dover's powder which I had administered might have been the cause of the fatal coma.

I will now give abstracts of the recorded histories of a number of cases of acute uræmia, which it seems to me go to show that the dangers attending the administration of opium in acute uræmia have been overrated.

These histories will also, I believe, enable us to judge to some extent of the effects of morphine administered hypodermically to patients suffering with uræmic intoxication. I shall detain you with only so much of these histories as bear upon the two questions before us.

Case I.—In the early part of March, 1868, a gentleman, fifty-eight years of age, who for two or three years had known from his physician that he had Bright's disease of the kidneys, came under my care in a condition of general anasarca, suffering almost constantly with headache, inability to sleep, and a restlessness which compelled his attendants to be constantly moving him from one position to another.

On examination I found cardiac hypertrophy, slight pulmonary œdema, general anasarca, albuminous urine of low specific gravity, which contained finely granular and hyaline casts. The quantity of urine passed on the first day of my attendance was forty-four ounces. He suffered somewhat from dyspnœa, but it was not extreme. On the third day of my attendance, his urine became markedly diminished in quantity—his headache, restlessness, and dyspnœa, were greatly increased, muscular twitchings were present, his pulse became accelerated and irritable in character, beating 120 in a minute,

his surface dry and hot (the temperature was not taken). Fearing convulsions, I had dry cups applied over his kidneys, and plied him with diaphoretics and hydragogue cathartics without any apparent relief. Death becoming imminent, I asked Dr. Metcalf to see him with me. At Dr. Metcalf's suggestion, and under his direction, I administered to him my first hypodermic injection of morphine to a patient with uræmia, expecting to see its administration followed by a fatal coma. To my astonishment, my patient, soon after its administration, passed into a quiet sleep, from which he was easily aroused, during which he perspired freely. On the following day, he reported himself as greatly relieved; his urinary secretion was reëstablished, and he was able to take and retain large quantities of milk. For six weeks I injected daily into this patient from twenty to thirty drops of Mag. sol. morphine hypodermically, and gave one-half ounce of infusion of digitalis twice a day. During this time, not only was he relieved of most of his distressing symptoms, but his improvement was so decided that he was able to walk about his rooms and go out to ride. In about two months he went into the country, and I only heard from him occasionally. His dropsy entirely disappeared. Whether the plan of treatment mentioned was continued or not I do not know. He died the following August, so far as I could learn, in a state of collapse, following an attack of what seemed to be cholera morbus. This case taught me that in some cases of Bright's disease with marked uræmic symptoms morphine could be administered hypodermically not only with safety, but with apparent advantage. Since that time I have occasionally used hypodermic injections of morphine in the treatment of patients with Bright's disease, especially when the premonitory symptoms of acute uræmia were present, as well as during the active manifestations of uræmic intoxication, and, so far as I am able to judge, its administration has been uniformly followed by good results. In no instance am I aware that I have caused a fatal narcotism.

The following history shows the effects of an hypodermic injection of morphine in uræmic intoxication, coming on during the acute stage of parenchymatous nephritis:

Case II.—J. B., a young man, twenty-three years of age, of temperate habits, free from hereditary or acquired tendency to disease, early in February, 1869, came under my care with acute Bright's disease. Three weeks previous he had been thoroughly chilled after an exposure of two or three hours on one of the docks on a damp, chilly day.

From that time he did not feel well, suffered more or less from headache, loss of appetite, and nausea. Ten days before I first saw him, he had noticed his face swollen on rising; at the same time he noticed that his urine was scanty and darker than usual. He had sent for me to relieve the pain in his head, which he described as terrible.

On examination I found his feet and legs, as well as his face, slightly œdematous; his pulse was 110, and irritable in character; skin hot and dry. He said that he had passed no urine since the previous night, but at my request voided about four ounces of smoky-looking urine which was highly albuminous; it was not examined microscopically. I ordered him to be dry-cupped over the lumbar region, a hot-air bath, and a large saline cathartic.

When I next visited him, twenty-four hours after, all his previous symptoms were aggravated. The œdema was increased; he had passed little urine, none for ten or twelve hours, and his bladder was empty; pulse 120, headache still severe, vision imperfect, was restless and at times delirious; dyspnœa not severe.

As the hot-air bath had produced very little diaphoresis, and his bowels had not moved, I ordered him one grain of elaterium, to be followed by an enema in four hours, and half an ounce of the infusion of digitalis every two hours.

At four o'clock the next morning, six hours after, I was summoned to him with the statement that he was in a convulsion. When I reached him he was semi-comatose; his friends said his convulsion lasted twenty minutes. His bowels had not been moved. I immediately administered a large enema of spirits of turpentine and oil, which was soon returned without any fecal discharge. His muscles began to twitch, he be-

came restless, his skin was dry and hot; pulse 130 and small. Fearing another convulsion, I administered hypodermically fifteen drops Mag. sol. morphine. Gradually the muscular twitchings ceased, he became quiet, and passed into a heavy sleep. I remained with him. In about two hours after the administration of the hypodermic, his surface was covered with a profuse perspiration, and his breathing became more natural. He could be aroused, and would swallow when fluid was placed in his mouth; four hours after, with a catheter, I drew off five or six ounces of highly-albuminous urine, which contained blood and granular casts. Six hours after, I commenced the administration of the infusion of digitalis, a tablespoonful every two hours; he was sleeping quietly, perspiring freely, could be easily aroused. I then left him.

At my next visit, ten hours after the administration of the hypodermic injection, I found him sleeping—skin moist, pulse 100, could be easily aroused and drank freely of milk. At my request he passed six or eight ounces of urine; his bowels had moved freely twice.

From this time, under the daily administration of digitalis and mur. tinct. ferri, and a milk diet, he went on to complete convalescence.

This was a somewhat rare case of acute parenchymatous nephritis occurring independent of any known blood-poison.

It shows, in a striking manner, how difficult it is to get the action of diaphoretics, diuretics, and cathartics, when the symptoms of acute uræmia are present in such cases, as well as their failure to prevent the occurrence of convulsions. The administration of a full dose of morphine, at apparently the most unpromising period in the history of the case, not only seemed to prevent an impending convulsion, but aided in the establishment of a saving diaphoresis and diuresis.

The two following histories have been furnished me by Dr. H. B. Millikin, late House-Physician to Bellevue Hospital.

Case III.—J. C., a German dyer, seventy years of age, was admitted into Bellevue Hospital, April 22d, at 6 p. m.

This patient was brought to the hospital in a semi-comatose

state, with almost complete suppression of urine, after successive convulsions occurring at intervals for twenty-four hours. He began to manifest the characteristic symptoms of Bright's disease about two months before the occurrence of the convulsions, and at the time of their occurrence was undoubtedly already in the second stage of parenchymatous nephritis.

The first hypodermic injection of morphine arrested the convulsive movements, and was followed in two hours by profuse diaphoresis. Two hours after, a second hypodermic injection was administered, and the internal use of digitalis was commenced, but there was no evidence of a return of the urinary secretion until twelve hours after the second hypodermic injection was administered; still, during this period of urinary suppression, there was no return of the convulsions.

After the reëstablishment of the urinary secretion, his convalescence was rapid, at least up to the time of his leaving the hospital. His subsequent death in a convulsion is evidence of the progressive character of his renal disease.

The abstract of the following case is taken from the notebook of Dr. Katzenbach, House-Physician to Bellevue Hospital during the service of Dr. Flint:

Case IV.—This was a well-marked case of chronic parenchymatous nephritis, which was treated with cups over the lumbar region and hydragogue cathartics for a month before the occurrence of the first convulsion; ten minims of Mag. sol. morphia were administered hypodermically immediately after the first convulsion, while the patient was comatose, which seemed to have little or no effect, for the convulsions continued at intervals for twenty-four hours, and, although one and a half grain of elaterium was administered during this time, there was no cathartic action from the bowels until some time after the convulsions ceased. He was put on diaphoretics, diuretics, and cathartics. One month later another series of convulsions and coma occurred, lasting forty-eight hours, during which time forty minims of Mag. sol. morphia were administered hypodermically—ten minims after each of the first convul-

sions—which had the effect of temporarily arresting the convulsions and causing profuse diaphoresis.

After the last hypodermic injection, he fell into a natural sleep, and the fall of the pulse and of the temperature, with profuse diaphoresis, marked the commencement of his recovery.

Morphine was not fully tested in this case, for it was not given immediately preceding the convulsions, and other remedial agents were employed in connection with it; it was given, however, in large quantities, and did not produce narcotism.

The full histories of the two following cases of uræmic convulsions (which we give here only in abstract) have been kindly furnished me by my friend Dr. B. W. Dudley:

Case I.—This patient, when first seen by Dr. Dudley, was probably in the acute stage of the inflammatory form of Bright's disease—there was almost complete arrest of the excretory function of the kidneys, and the premonitory symptoms of acute uræmia were well marked.

The first convulsion was a severe one, and, in accordance with the usual history of such cases, was sure to be followed by a second. The first large hypodermic injection of morphine seemed to arrest the impending convulsion for about three hours, when the administration of a second controlled all muscular twitchings, established a profuse diaphoresis, and so far overcame the toxical effects of the urea that under the free administration of digitalis for four hours the urinary secretion was reëstablished.

This case also shows the effects in acute uræmia of morphine hypodermically and digitalis internally, uninfluenced by any other remedial measures.

Case II.—The second case was an exceedingly interesting and remarkable one, not only on account of the large amount of morphine administered during twelve hours, but from the fact of recovery after so many and so severe convulsions. The patient had for years suffered moderately from uræmic poisoning. But sudden renal congestion from exposure to cold caused a

complete arrest of the functions of the already damaged kidneys, which was almost immediately followed by the symptoms of acute uræmia.

During the first eight hours of his convulsions, he was faithfully plied with diaphoretics and cathartics without obtaining the action of either, during which time his convulsions increased in severity, and his case seemed hopeless. (The exceedingly large doses of morphine borne by this patient may have been due somewhat to the fact that some years before he became accustomed to its effects.) The first large hypodermic injection of morphine seemed for a time to arrest the progress of the disease. But the exciting cause of the convulsive seizure was so potent that the struggle between it and the arresting agent became a desperate one, and not until the system was overwhelmed with the morphine did its controlling action show itself. During the active period of the convulsion, the temperature and pulse were both high. The fall in temperature and pulse marked the subsidence of the convulsive paroxysm.

From this case as well as others that have come directly under my own observation, I am of the opinion that the rule which is to govern us in the hypodermic administration of morphine in uræmic convulsions is to give it in quantities sufficiently large, and frequently repeated, to control the convulsions. Especially will this hold true in uræmic eclampsia. Dr. Dudley boldly pushed the remedy which he believed was the only one that gave his patient any chance of recovery, and his boldness apparently saved the life of his patient. As regards the efficacy of hypodermics of morphine in the treatment of uræmic eclampsia, my own experience has been limited to a single case, the main points of which are as follows:

Case.—Early in April, 1870, Mrs. W., a young woman, twenty-two years of age, in the eighth month of her first pregnancy, consulted me in regard to a severe pain in her head, from which she said she had suffered for two or three weeks. On examination, I found her feet and legs œdematous, her urine highly albuminous, containing granular, epithelial, and fatty casts.

I warned her family of her danger, and prescribed for her the daily use of mur. tinct. ferri and digitalis, with the frequent application of dry cups over the lumbar regions. Her headache was somewhat relieved, and she passed on to the commencement of her labor without any other unpleasant symptoms. The albuminous urine and œdema continued.

I was called to see her about six hours after the commencement of her labor. Every thing seemed to be progressing favorably. The os uteri was dilated to the size of a half-dollar, but was rigid.

After watching the case for an hour or more, I noticed that she was becoming restless, her pulse became slightly accelerated and irritable in character. All at once she began to complain of pain in her head, at times screaming out on account of its severity.

I introduced a catheter into the bladder, but could get only about half an ounce of urine—was told she had not passed urine for ten or twelve hours.

Suddenly she passed into a convulsion, which lasted about ten minutes, after which she became partially comatose. The labor continued regular, and the process of dilatation went on rapidly. In about half an hour she began to be restless. Fearing another convulsion, I administered hypodermically fifteen minims of Mag. sol. morphia. Immediately her restlessness ceased, and her labor progressed more rapidly than before. Two hours after the hypodermic injection, the os being fully dilated, and the head well down in the pelvis, I applied forceps, and easily completed the delivery of a small child. She then passed into a quiet sleep, her pulse falling to 100. I remained with her an hour or more, and left her sleeping quietly.

In about two hours I returned and found her just passing into another convulsion; immediately I injected ten minims of Mag. sol. morphia. The convulsion lasted fifteen minutes, and she passed into a more profound coma than before. In one hour the muscles of her right arm began to twitch, and I injected ten minims more of Mag. sol. morphia. Her breath-

ing soon became stertorous, her pulse almost imperceptible, and I thought her dying.

Gradually the stertor passed away—her breathing became like that of one in quiet sleep—her pulse gradually diminished in frequency and improved in character—her skin became bathed in profuse perspiration. I did not allow her to be disturbed, and, after remaining four hours in this condition, she roused to consciousness and asked for a drink, which she readily swallowed.

Her urinary secretion was soon reëstablished; she passed on to a complete but slow convalescence, and entirely recovered.

In this case I did not use chloroform, or any of the remedies usually employed in the treatment of uræmic eclampsia, for this reason: that the only two cases of uræmic eclampsia that had occurred in my practice had terminated fatally after the usual remedial agents had been resorted to (in one, general bleeding was twice practised by advice of the late Dr. Gilman).

I am aware that a single case proves very little, in regard to a plan of treatment, but this case, it seems to me, shows the power of hypodermics of morphine to control muscular spasms, excite diaphoresis, and perhaps diuresis, in a patient in uræmic eclampsia.

In an August number of the *Medical Record* of 1868 I find a much stronger support of this plan of treatment in uræmic eclampsia.

In a paper on the "Diagnosis of Albuminuria in Pregnancy," etc., by Dr. F. D. Lente, is a letter from Dr. White, of Fishkill, which reads as follows (*vide* vol. iii., page 265):

"Case IX.—Dr. White says: 'I was called to visit her (Mrs. M——) at 5½ p. m. on Friday, February 22, 1867. I found her in a comatose and insensible condition, having had two convulsions before I arrived. She had been complaining during the day of what they call sick-headache up to the time the convulsions came on. The pulse was full, skin hot, bowels constipated; very considerable œdema (of lower extremities).

The time of her expected confinement had arrived, but no evidence of labor. I bled her sixteen ounces from the arm, applied ice to head and mustard to the neck, and gave fifteen drops of Mag. sol. morphia. Within half an hour she had another convulsion, when I sent for you (Dr. Lente); and about the time you arrived she had another, when you injected the arm (with morphine one-half grain), and had her bowels moved by an enema, and the bladder evacuated by the catheter; by which operation we obtained but a small quantity of urine, there evidently being but little secretion. After this she had no more convulsions. Whenever she became uneasy, and gave evidence of their return, she had an injection *per anum* of one grain of morphine; this was repeated every six or eight hours, three times, when all unpleasant symptoms disappeared.

"'The kidneys became active soon after she was fully under the influence of the morphine. During the day and night of Saturday, she voided seven and a half pints of urine of a natural color. After this I made no note of the quantity, but remained with her until three o'clock Sunday afternoon. The following morning, at eight o'clock, I was called to her, and found her in labor; and at eleven o'clock A. M. she was delivered of a full-sized child, with no unpleasant symptoms during or after the labor. She recovered rapidly, and has enjoyed uninterrupted good health since. I saw her this week, and found her in perfect health, and in the sixth month of pregnancy.'"

In connection with this case, Dr. White makes the following remarks:

"I made a verbal report of this and five other cases of puerperal convulsions (that I have had within the last two years, all treated with morphine and all terminated favorably) to our County Medical Society, at the semi-annual meeting in January last, and two cases were reported by Dr. G. L. Sutton, in which I was in attendance with him, which were treated with morphine and resulted favorably. It provoked a very spirited discussion by the members of the Society, a ma-

jority of whom had treated their cases with chloroform, bleeding, and veratrum. But they were free to acknowledge that the mortality was fearfully great."

In a letter from Dr. Lente, dated March 3, 1873, in answer to inquiries made by me in regard to his treatment of uræmic eclampsia, he makes the following statement: "I remember one very bad case of convulsions in a primipara, some nine or ten years ago; they commenced before any signs of labor appeared, and continued for several days before the pains set in, and also for a couple of days after labor, if I remember rightly. I was called in consultation. Chloroform was administered at first, and continued until it seemed as if the patient would die each time, while under the combined influence of the convulsion and the remedy. I had not then the experience with morphine in these convulsions which I have had since, and hesitated to advise it; but, being driven to it, I gave hypodermic injections, with the effect of promptly relieving the paroxysm, and relieving the patient for several hours. These were continued for several days. The patient recovered well and has been well since, having borne other children, with no recurrence of albuminuria."

These histories and their accompanying statements go far, it seems to me, to place hypodermic injections of morphine among our most reliable agents in controlling this terrible form of acute uræmia; and it would appear that, if a large hypodermic injection of morphine be administered at the onset of uræmic eclampsia, and repeated whenever the premonitions of a convulsion are present, we offer these distressing cases the best chance of recovery.

I have no experience in the treatment of acute uræmia following surgical operations on the urethra.

The only patient I ever saw with uræmia under such circumstances was in a moribund condition when I was first called to him. From his history I should infer, if morphine is ever to be resorted to in this class of cases, it should be given as soon as the premonitory symptoms appear, not for its power of preventing or controlling muscular spasms, but as an eliminator.

I am not aware that any case of this class has been treated with hypodermic injections of morphine.

In the histories of the ten cases given, I think we find answers to the two questions asked at the commencement of this discussion:

1. That morphine can be administered hypodermically to some if not to all patients with acute uræmia, without endangering life.

2. That the almost uniform effect of morphine so administered is: (1.) To arrest muscular spasms by counteracting the effect of the uræmic poison on the nerve-centres; (2.) To establish profuse diaphoresis; (3.) To facilitate the action of cathartics and diuretics, especially the diuretic action of digitalis.

Thus morphine, administered hypodermically, becomes a powerful eliminating agent.

The rules which are to govern its administration are as yet not well defined. My own experience would teach me to give small doses at first, not to exceed ten minims. If convulsions threaten, and a small dose does not arrest the muscular spasms, it may be increased to twenty minims, and the hypodermics may be repeated as often as every two hours. It must be given in sufficient quantities to control convulsions; neither the contraction of the pupils nor the number of the respirations is a reliable guide in its administration.

It has not been my purpose, Mr. President, in this paper, to discard all, perhaps none, of those means which have been relied on for the relief of patients in acute uræmia, but to bring to the notice of the profession the fact that, in a certain proportion of cases (if not in all) of acute uræmia, hypodermic injection of morphine will not only control muscular spasms, but aid in establishing the eliminating processes, and thus become another means of saving life in these too often fatal cases.

The Academy then adjourned.

Stated Meeting, April 17, 1873. Dr. Austin Flint, President, in the Chair.

After the transaction of the usual routine of business, the President called for the reports of special committees, when—

The committee appointed to prepare resolutions on the demise of Dr. J. C. Nott made their report—prefacing which, Dr. Purple, Vice-President, gave the following brief sketch of the life and labors of the deceased:

Josiah Clark Nott, M. D., whose death we are called upon to notice this evening, was a son of Abraham Nott, a distinguished Judge of South Carolina, and was born in Columbia, S. C., March 31, 1804. He received a collegiate education, and graduated in Arts at South Carolina College in 1824; and in Medicine at the University of Pennsylvania in 1827, having defended a thesis on "Costiveness."

Immediately on graduating he was appointed Demonstrator of Anatomy under Dr. Philip Syng Physic, who was then Professor of Anatomy in the university. For two years he discharged the duties of this appointment, when, his health failing and showing indications of pulmonary tuberculosis, he removed South to the place of his birth. Here he devoted himself to the practice of his profession, and spent his leisure hours in the study of natural history. In 1835 he visited Europe, and on his return, in 1837, settled in the practice of medicine in Mobile, Ala. Here he became distinguished in the departments of both medicine and surgery. In 1857 he was appointed Professor of Anatomy in the Medical Department of the University of Louisiana; and subsequently became the managing spirit in establishing the Medical College of Alabama, an institution which was endowed with $50,000 by the State Legislature, and in which he occupied the professorial chair of Surgery for several years.

His literary labor in the various departments of science, and particularly in medicine and ethnology, immortalized his name. He contributed largely to the medical periodicals of the day; and from his pen the following works have been published:

In 1831 he translated from the French of M. A. Goupil, D. M. P., "An Exposition of the Principles of the New Medical Doctrine, with an Analysis of Theses sustained on its Different Parts," to which he added a short "Essay on Leeches." This work was published the same year at Columbia, S. C.

In 1844, "The Natural History of the Caucasian and Negro Races."

In 1849, "The Connection between the Biblical and Physical History of Man."

In 1850, "The Physical History of the Jewish Race."

In 1854, "Types of Mankind; or Ethnological Researches based upon the Ancient Monuments, Paintings, Sculptures, and Crania of Races," etc.

In 1857, "Indigenous Races of the Earth; or, New Chapters of Ethnological Inquiry," etc.

The last two works were prepared in connection with Geo. R. Gliddon, formerly United States consul at Cairo, and other distinguished gentlemen then engaged in similar pursuits.

In 1866, "Contributions to Bone and Nerve Surgery." This work was privately printed and distributed mainly among his personal friends. It was principally made up from his personal observations and experience in the late war of the rebellion.

In 1868 he came to this city to reside, engaging actively in the practice of his profession. In 1871 he was elected a Fellow of this Academy. Last spring he was seized with hæmoptysis, which he regarded as having a tuberculous origin. A repetition of the hæmorrhage compelled him to leave the city for the summer. Late in the autumn he returned and removed to Mobile, Ala., hoping to obtain relief from the pulmonary trouble which finally closed his earthly career on the anniversary of his birth, March 31, 1873, aged just sixty-nine years. Of his distinguished labors among us, while resident in this city, there are those present who can speak better than myself.

The resolutions, prepared by Drs. Roberts and Purple, were then read and adopted, after which

Dr. W. T. Lusk read a paper on the "Etiology and Indications for the Treatment of Irregular Uterine Action during Labor." (Published elsewhere.)

The Academy then adjourned.

STATED MEETING, MAY 1, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

Dr. Henry Wentworth Ackland, of Oxford, England, was elected a Corresponding Fellow. Dr. A. M. Hamilton was elected a Resident Fellow.

The President announced the reception of the *Edinburgh Medical Journal* for April, 1873, and the "Report of the Board of Health of New York City, for 1871," as contributions to the library.

A letter from Dr. Dichiara, of Palermo, Italy, thanking the Academy for his election as Corresponding Fellow, was read by Dr. Caro.

Dr. Brown-Séquard, by invitation, made some remarks on the "Importance of Frequent Auscultation and Percussion of the Chest, in Cases of Organic Brain-Disease."

The Academy then adjourned.

STATED MEETING, MAY 15, 1873. DR. S. S. PURPLE, FIRST VICE-PRESIDENT, IN THE CHAIR.

Drs. C. R. Bogert, John J. Mason, C. F. Rodenstein, F. LeRoy Satterlee, and L. M. Yale, were inaugurated Resident Fellows.

The President announced the reception of the *Edinburgh Medical Journal* for May, 1873.

Dr. W. M. Chamberlain read a paper entitled "Remarks upon the Principles involved in the Mechanical Treatment of Uterine Displacement." (Published elsewhere.)

Dr. Russel then read the following paper:

ON THE MORTALITY IN THE VARIOUS STATES OF THE UNION.

By CHARLES P. RUSSEL, M. D.

Mr. President and Fellows of the Academy:

I propose to glance briefly at the general features of recent diseases throughout the United States, and to point out some special local peculiarities as exhibited by death-rates in particular States. It must be acknowledged that even exact figures of mortality do not indicate, with positive accuracy, prevailing conditions of the public health, especially in the case of diseases subject to constant fluctuations of type. They are, however, indices which point unerringly in the right direction, and as such they are entitled to our most careful consideration. Moreover, they are our sole means at present for approximate investigation of national disease.

Among our English kinsmen across the Atlantic, there has existed for many years a uniform and comprehensive system of death-registration. Thus, within a brief period after the outbreak of an epidemic, its mortuary figures from every quarter reach the central bureau in London, where they are systematically arranged and published at once. The character of the morbific storm is studied, and its course predicted with almost as much promptness and certainty as each approaching disturbance of the elements is foretold and described in Washington from a comparison of manifold meteorological phenomena. In the same manner, whatever peculiarities may characterize the mortality by sporadic and endemic affections at different seasons, in various portions of the country, are observed and converted into numerical expressions for analysis.

It is unfortunate for the cause of medical and sanitary science that no similar system has been established in the United States. In our population of nearly forty millions, it is estimated that about seven hundred thousand deaths occur annually; and yet, except in the case of our large cities, we are as ignorant of our fatal diseases as we are of those which cut off the population of China.

This deficiency is, in a measure, compensated for by the periodical enumeration of causes of death at each national census. Although for obvious reasons such enumeration must be defective, both as regards the actual causes themselves and the number dying within the census year (the returns being computed as forty-one per cent. less than the true number), still, the same sources of error and the same elements of truth obtaining as a rule in every section, the results of comparisons between different portions of the country contain much less of fallacy and much more of fact than might be anticipated. For the last census year, ending June 1, 1870, nearly half a million deaths were collected and appropriately arranged by the Census Bureau, in tables referring both to the country as a whole and to separate States and Territories. Even a superficial analysis of so many deaths cannot but prove interesting; and it is my intention to point out whatever of importance has occurred to me during a hasty examination of said tables. The figures which I shall present must not be regarded as representing actual death-rates, but rather as approximate results of inquiries into comparative proportions of mortality in different parts of the Union.

I shall conclude with some remarks upon city death-rates throughout the world.

In contemplating the general causes affecting the health of a nation, it would seem proper that some stress should be laid upon the circumstances of morals, industrial pursuits, customs and modes of life, material prosperity, race, peculiarities of soil and climate, and even political tendencies and events. The purpose and limits of this paper, however, preclude my entering into such considerations or discussing subjects some of which, with our present knowledge, may be said to have scarcely emerged from the domain of speculation. While it must be admitted that, within certain limits, those influences modify more or less the production, nature, or course of special diseases, the difficulties of such abstruse investigations are vastly increased by the heterogeneous and fluctuating char-

acter of our population, and the vast extent of territory over which it is spread.

With regard to local climate and meteorological conditions, I shall submit, without comment, a few facts which will only be found to concur with well-established truths. Attempts have been made to divide the States into groups representing sectional climatic characteristics, with the object of proving their influence upon health. Such a plan, adopted in the publication of the census of 1860, has been very properly abandoned in that of the last census, whose accomplished superintendent, General Francis A. Walker, thus expresses himself on the subject :

"It is evident that, if the States and Territories of the United States are to be classified by districts according to an assumed unity of vital conditions within each district, it is a matter of absolute necessity that the propriety of the classification shall in every case be beyond question. Otherwise such an arrangement, instead of bringing out the truth, will only serve to conceal it."

Prof. John W. Draper remarks that "this grouping accords neither with the annual isothermals nor with those of summer or winter." He further adds : "Considering how imperfectly the meteorology of the continent is at present understood, any system of grouping dependent on it must be liable to fallacy. In ten years more, perhaps, such an attempt may possibly be executed."

The year of the last or ninth census ended on June 1, 1870, but it is only very recently, owing to the immense labor required for so vast a compilation, that the complete results embodied in the volume on Vital Statistics have been given to the public. Although they refer to a period of three years since, they are almost as interesting as though they represented present conditions, from which they cannot differ very materially.

The year mentioned was not marked by any general epidemic like those of small-pox and cerebro-spinal fever, which have since invaded almost the entire country. The former

disease, however, was remarkably fatal in some of the sparsely-settled Western regions. For example, its mortality in this city during the past year was greater than ever before recorded, being equal to nine deaths in every ten thousand inhabitants. But we find it reaching, in Arizona, the enormous figure of ninety-eight in the ten thousand; in Nevada, seven; in Missouri, six; and in Montana, five. These figures are probably very near the truth, as a death by small-pox is not apt to be forgotten or mistaken by the relatives. Sixty-nine per cent. of these deaths were of children less than ten years old, their proportion here being about 50 per cent. The lack of efficient vaccination among the remote settlers of the Plains would appear demonstrated by these figures.

The deaths by measles, scarlatina, diphtheria, croup, and whooping-cough, were distributed quite uniformly over the country. As they appear in the census, they present some peculiarities essentially different from their history in this city, which relate to the large proportion of adults and aged persons to whom they proved fatal. Measles is credited in the census with a total of 9,237 deaths, or 1.8 per cent. of the whole mortality. Of these deaths by measles, 516, or 5½ per cent., are given as having taken place between ten and twenty years of age; 762, or 8 per cent., between twenty and forty years; 247, or 2.6 per cent., between forty and sixty years; 60, or 6 per cent. between sixty and seventy-five years; 5 deaths between seventy-five and eighty years; 6 between eighty and eighty-five; and 1 between eighty-five and ninety. In this city, during the past five years, out of 136,854 deaths, 1,896 were referred to measles, or 1.4 per cent. of the total mortality—a proportion little less than that of the census. Only 11 deaths, however, occurred between ten and twenty years of age; 11 between twenty and forty years; 1 between forty and forty-five years; and none after.

The census attributes 20,320 deaths to scarlatina, being 4 per cent. of the total mortality. Of these, 1,474, or 7.2 per cent., are given as between ten and twenty years of age; 324, or 1 per cent., between twenty and forty years; 73 be-

tween forty and sixty years; 30 between sixty and seventy-five years; 1 between seventy-five and eighty; and 2 between eighty-five and ninety.

In this city during the past five years, out of 4,583 deaths by scarlatina, 3.3 per cent. of the total mortality, 157 deaths, or 3.4 per cent., took place between ten and twenty years of age; 94, or 2 per cent., between twenty and forty years; 5 deaths between forty and sixty years; 2 between sixty and sixty-five, and none after.

Diphtheria, according to the census, occasioned 6,303 deaths, or 1.2 per cent. of the total mortality. Of these, 476, or 7.5 per cent., are stated as occurring between ten and twenty years of age; 250, or 4 per cent., between twenty and forty years; 117, or 2 per cent., between forty and sixty years; 58, or 9 per cent., between sixty and seventy-five years; 8 between seventy-five and eighty years; 8 between eighty and eighty-five years, and 1 over ninety-five.

In this city, during the past five years, diphtheria caused 1,597 deaths, or 1.2 per cent. of the total mortality: 31, or 2 per cent., occurred between ten and twenty years of age; 26 between twenty and forty years; 14 between forty and sixty years; 3 between sixty and seventy-five years; and 1 between seventy-five and eighty years.

The census gives 10,692 deaths as the result of croup, or 2 per cent. of the total mortality. Of these, 82 took place between ten and twenty years of age; 26 between twenty and forty years; 15 between forty and sixty years; 9 between sixty and seventy-five years; 2 between seventy-five and eighty years; and 4 between eighty and eighty-five years.

In this city, during the past five years, there were due to croup 2,387 deaths, or 1.7 per cent. of the total mortality. Only 7 of these occurred between ten and twenty years of age; 6 between twenty and forty years; 1 between fifty-five and sixty, and none after.

Whooping-cough, according to the census, produced 9,008 deaths, of which 96 are given between ten and twenty years of age; 25 between twenty and forty years; 13 between forty

and sixty years; 6 between sixty and seventy-five years; 2 between seventy-five and eighty years; and 1 between eighty and eighty-five.

During the past five years in this city, out of 1,806 deaths by whooping-cough, none were recorded beyond ten years of age.

There is one fact characteristic of whooping-cough exhibited by both the figures of the census and of this city, viz., the immense preponderance of females over males in the fatal cases. According to the census it carried off 3,987 males, and 5,021 females. Within five years in this city it has caused the deaths of 777 males and 1,029 females. I am not aware that this peculiarity of whooping-cough has ever been accounted for.

The mortality by typhus fever was small. It was present, however, everywhere except in Arizona, Idaho, Montana, and Nevada. Typhoid fever, on the other hand, was a prominent and universal cause of death, its mortality amounting to 22,187, making it third in order of the most fatal diseases.

Idaho alone was exempt from it. As, however, only 50 deaths in a population of 15,000 were reported from that Territory, its figures may be dismissed as unworthy of consideration. For the same reason I shall now exclude Arizona, Wyoming, and Dakota, confining my observations to States or Territories having over 20,000 inhabitants. The death-rate of typhoid fever was highest in Maine, New Hampshire, Connecticut, North Carolina, Missouri, and New Mexico, reaching within a fraction of 10 in every 10,000 inhabitants. In Massachusetts, Delaware, and South Carolina, it ranged between 7 and 8; in California, Indiana, Illinois, Vermont, Georgia, and Louisiana, between 6 and 7; and in Pennsylvania, Maryland, Virginia, Kentucky, Tennessee, Florida, Texas, Oregon, Washington Territory, Kansas, Minnesota, and Michigan, between 5 and 6. It was least fatal in Utah, Colorado, and Nevada.

The marsh-miasmatic fevers, intermittent, remittent, and typho-malarial, form an important feature of the mortality

tables of the census. The first of these diseases is not mentioned as a cause of death in Colorado or Montana. Remittent does not appear in Montana, nor typho-malarial in New Hampshire, Rhode Island, Connecticut, Delaware, District of Columbia, West Virginia, New Mexico, Arkansas, Nebraska, Colorado, Oregon, or Washington Territory. These fevers altogether were most fatal in Florida, Louisiana, and Texas, where they cut off respectively, 12.6, 11.5, and 11.4 persons out of every 10,000. Arkansas comes next with 8.9, Mississippi with 7.6, Alabama with 7.5, followed by Georgia, Missouri, Kansas, and Nevada, with from 6 to 7 each. The region of the Gulf States and Southwest is thus perceived to be particularly deadly in miasmatic influences. Next in order of fatality we find New Mexico, the Carolinas, West Virginia, Tennessee, Kentucky, Illinois, and Indiana. Those States distinguished by the lowest mortality were, the New England and Middle States, Wisconsin, and Minnesota. In California there was a considerable ratio of mortality, diminishing easterly in Utah, and northerly in Oregon and Washington Territory, while toward the south, in New Mexico, it augmented largely. From the foregoing figures it would appear that geographical situation and climate are the most important elements in the production of the more formidable forms of these fevers. Since the census year, however, there has been noticed an evident extension of the subtile miasmatic agency, which engenders them, toward and over regions of the Middle and New England States hitherto entirely exempt from their influence. The great increase of mortality by such fevers in our own city must be ascribed as much to our participation in the effects of this invasion as to apparent local causes. While, in 1870, marsh-miasmatic fevers occasioned here 213 deaths, they amounted in 1871 to 288, and in 1872 to 348. Congestive chill, the variety always so common in the South, and until recently almost unknown here, has now become a not infrequent cause of death among us. Although in many instances these fevers have been clearly derived from terrestrial emanations, there have been other cases among residents of

populous and salubrious neighborhoods, in persons never, so far as known, exposed to what have been regarded as the essential causes of such diseases.

Cerebro-spinal fever and yellow fever are noticed in the census as occasional causes of death. The former existed to some extent in New York, Pennsylvania, Ohio, West Virginia, Indiana, Illinois, Kentucky, Georgia, Alabama, Louisiana, and Texas—having, therefore, pretty well-defined limits. Yellow fever was not found in any Territory except Utah, where one death was noted, and the District of Columbia, where there were two. It was given as a cause of death in every State except Oregon, Nevada, Kansas, Nebraska, Minnesota, Wisconsin, Michigan, and West Virginia. It was most fatal in the following States: Florida, where it caused 45 deaths; Maryland, 21; Maine, 17; Massachusetts and Alabama, 12 each; Louisiana, Texas, and New York, 7 each; Georgia, Mississippi, Missouri, and Pennsylvania, 5 each.

Diarrhœal diseases were most fatal in the following States and Territories: Massachusetts, Connecticut, Rhode Island, New York, New Jersey, Pennsylvania, Delaware, Maryland, Virginia, District of Columbia, Florida, Mississippi, Louisiana, Illinois, Kansas, Utah, and New Mexico. Most of the principal cities are contained in these States, a fact doubtless determining their high death-rate. I include, under the term "diarrhœal diseases," cholera infantum, cholera, diarrhœa, and dysentery.

Puerperal diseases occasioned the highest relative mortality in Utah—a somewhat significant fact—where nearly 7 in every 10,000 of the female population thus perished. Texas came next, with 6.4. No other State approached these figures. The mortality by puerperal diseases was extremely low in Maine, New Hampshire, Vermont, New Jersey, Delaware, Ohio, and Tennessee. In this city, during the past five years, the average annual death-rate from these causes has been about 1 in each 10,000 of the female population, or one-seventh of that of Utah during the census year.

The deaths by syphilis were so few that their actual num-

ber will give a better idea of its mortality than a fractional death-rate. Its deaths were most numerous in the following States: New York, 116; California, 50; Pennsylvania, 46; North Carolina, 37; Georgia, 36; Louisiana, 33; Massachusetts, 32; Ohio, 31; and Illinois, 28. None were reported in Vermont, Rhode Island, New Jersey, West Virginia, Iowa, Minnesota, Nebraska, Montana, Nevada, Utah, and Washington Territory.

In the whole Union 1,161 males and 249 females are recorded as having fallen victims to the vice of intemperance. Over 75 per cent. of these deaths were furnished by States containing only 45 per cent. of the population, but including the nine principal cities—Maryland only being excepted and standing quite low on the list.

Rheumatism attained the highest death-rate in New Mexico, viz., 4.2 deaths in each 10,000 inhabitants. Montana came next with 1.9; Florida with 1.7; District of Columbia with 1.4; Louisiana with 1.1; and Virginia with 1. The mortality of the last-mentioned State was most closely approached by that of North Carolina, Maryland, Tennessee, Oregon, New Hampshire, and Massachusetts. It will be noticed that the two greatest death-rates for this disease were given by New Mexico and Montana.

The record of mortality by cancer affords some singular facts. It was most fatal in New Hampshire and Massachusetts, where it destroyed nearly 4 in every 10,000 inhabitants; in Maine, 3; in Vermont, 3; in Connecticut, 2.9; in Rhode Island and New York, 2.7; in Pennsylvania, 2.2; in Ohio, New Jersey, and Maryland, 1.6; in Delaware, 1.4. With the exception of Louisiana, where cancer caused 2 deaths in each 10,000 inhabitants, the above are its highest death-rates; and they exhibit a scale of mortality descending from the extreme Northeastern States by regular intervals toward the South and West. Another noticeable feature of this disease is the very large proportion of deaths among the foreign population as compared with the native, the proportion being among the former 2.5 and among the latter 1.1 in each 10,000 per-

sons. In this city during the past five years, a much greater discrepancy has appeared, the average annual death-rate by cancer having been 1.6 in each 10,000 natives, and 6 in each 10,000 foreigners. Considering how large a proportion of the native population consists of children not usually subject to cancerous affections, this difference would not appear so great between the adult native and foreign population, but would still be sufficiently striking.

The mortality by consumption exhibits, as a rule, such characters as might have been anticipated; its greatest death-rate being furnished by regions adjacent to the North Atlantic seaboard, and its lowest by those of the warmer southern latitudes and the extreme Western States east and south of California. That of California is high, owing perhaps to the numerous deaths of invalids from other States. Massachusetts gives the highest death-rate, viz.: 35.3 in each 10,000 inhabitants, followed by the District of Columbia, with 33.6, and Maine with 31.8. The other States in which a high mortality by consumption occurred were in order as follows, their death-rate ranging from 30 to 20 in each 10,000 inhabitants: New Hampshire, 29.9; New York, 26.4; Rhode Island, 25.4; Delaware, 23.7; Connecticut, 22.7; California, 22.2; Vermont, 21.6; Maryland, 21.5; Pennsylvania, 21.2; New Jersey, 20.9.

The lowest death-rates by consumption prevailed throughout the States along the Atlantic and Gulf, with the exception of Louisiana, from North Carolina to Mexico, as well as in Minnesota, Nebraska, Arkansas, Nevada, Montana, Colorado, Utah, and New Mexico, their mortality varying from 10 to 4.9 in each 10,000 inhabitants, New Mexico presenting the most favorable figures.

As usual, the number of females dying by phthisis was largely in excess of the number of males, being as 35,925 against 33,971. Among the native population over ten years of age this disease cut off 23.5 in every 10,000 people, and among the foreign population over ten years old it cut off 29.6 in every 10,000. The whites suffered to the extent of 18.4

in the 10,000, and the negroes 15.9. Among the principal foreign nationalities the proportion was as follows: Irish, 40 in the 10,000; French, 27.6; Germans, 24.4; English, Welsh, and Scotch, 22; and Scandinavians, 16.2.

In examining the mortality tables of this city, I have found that the Irish and Germans, who together contribute nearly 60 per cent. of our deaths by consumption, exhibit a very marked distinction as regards the proportion of the sexes dying by this disease. During the past five years it has caused the deaths of 7,992 Irish and 3,105 Germans. Among the Irish the percentage of female deaths has been 53, and of male deaths 47; while among the Germans the females have furnished only 35½ per cent. against 64½ per cent. males. There are no tables of the census by which I might verify this fact, if it be a general one; but it may possibly be peculiar to large cities where it may be explained by the essentially different habits of the two races. In-door and sedentary occupations, which are notoriously conducive to the development of consumption, are almost universal among the German male population of our cities, on the one hand; and on the other the lower classes of German women are more temperate and regular of habit than the corresponding order of Irish.

Next to phthisis, pneumonia was the most prominent cause of death, being credited with 40,012 deaths. It prevailed most extensively in a portion of the Southwest, embracing the contiguous States of Texas, Arkansas, Louisiana, Kansas, Missouri, Mississippi, and Alabama, its death-rate varying between 18 in each 10,000 inhabitants of the first-mentioned State, and 15 in each 10,000 of the last. It was least fatal in Washington Territory, Oregon, Michigan, Iowa, Wisconsin, Minnesota, Montana, and Colorado. It is thus seen to have well-defined limits of fatality and benignity, which, however, are probably not constant, determined by climate and season, but varying at intervals under influences of more or less epidemic character, as we have frequently noticed in this city. The negroes suffered from this disease very much more than the whites, 16 of the former and 10 of

the latter having died from it in every 10,000 of their respective population.

Bronchitis was not nearly so fatal as pneumonia, having only about one-tenth as many deaths referred to it. It caused more deaths proportionately in New York and Louisiana than elsewhere, but for the rest it was spread universally over the country.

Among the principal diseases of the nervous system, apoplexy and paralysis were most fatal in the New-England States, in the following order: New Hampshire, Massachusetts, Connecticut, Vermont, Rhode Island, and Maine, their death-rate in 10,000 ranging between 7.4 in the first and 5.5 in the last. Inflammatory cerebral affections, on the contrary, were distinguished by a very low mortality in every one of those States. In New York, Pennsylvania, Ohio, Maryland, District of Columbia, Virginia, Louisiana, and California, both of these varieties of disease were quite fatal. In Florida inflammatory cerebral affections carried off nearly 12 out of every 10,000 persons, while apoplexy and paralysis were rare causes of death. A corresponding mortality was noticed in all the other Southern and Southwestern States and Territories not mentioned, as well as in Indiana and Illinois. Thus, in the case of certain disorders of the nervous system, a very clear distinction exists between the more Southern States and New England—a distinction which disappears in passing into the intermediate sections. It might be inferred that an explanation of this difference could be found in some peculiarity of the negro race so largely represented in the South; and in fact such is the case in a measure, as the census shows 1 negro to 6 white deaths by meningitis and encephalitis, while only 1 negro to 16 whites died of apoplexy and paralysis. Climate, occupation, and habit, however, are doubtless largely concerned in the production of the results stated.

Diseases of the organs of circulation were most fatal in California, where they destroyed 7.7 lives out of every 10,000. New Hampshire and Massachusetts came very close after, followed by New York, Louisiana, Vermont, Connecticut, Pennsylvania, and District of Columbia. Their death-rate

was least in Arkansas and Nebraska, and was generally very low in the extreme Western States east of the Rocky Mountains.

As regards diseases of the liver, there is nothing particularly noticeable in the census-tables.

Diseases of the kidney were by far most fatal in New York, next in order coming Massachusetts, Colorado, Connecticut, California, Maine, Vermont, Pennsylvania, New Hampshire, and New Jersey. As a rule, the mortality was uniformly low in the South and West, and high in the Middle and Eastern States.

Diabetes was remarkably prevalent in Virginia, where it caused 140 deaths. Elsewhere in the South it was rare, but in the rest of the States it was quite a common cause of death.

I have now made a cursory examination of the principal definite causes of death in the United States as exhibited by the tables of the last census, with the exception of those due to violence, which being essentially confined in a great measure to cities, and being, moreover, very vaguely described, offer but little of interest.

With regard to nationality and race, the death-rate of the native population was given in the census as 13 in each 1,000 against 11.8 foreign. The principal foreign nationalities furnished the following death-rates: Irish, 14.5 in each 1,000; French, 14; natives of Great Britain, 11.5; Germans, 11; Scandinavians, 9.3. There died 13.8 in each 1,000 colored, and 12.5 in each 1,000 white inhabitants.

Of the total mortality 41 per cent. occurred among children under five years old.

The annexed table exhibits the most fatal and least fatal month in the various States:

Most and Least Fatal Months in the States and Territories of the United States during the Year 1870.

Most Fatal Months.		Least Fatal Months.	
March.	*May—(continued).*	*February.*	*October.*
Maine.	Mississippi.	Colorado.	Montana.
Vermont.	Louisiana.	*June.*	New Jersey.
New Hampshire.	Texas.	Maine.	*November.*
New York.	New Mexico.	New Hampshire.	New York.
New Jersey.	Wisconsin.	Massachusetts.	Pennsylvania.
Arkansas.	Colorado.	Rhode Island.	Maryland.
Ohio.	*August.*	Connecticut.	Dist. of Columbia.
Indiana.	Massachusetts.	Ohio.	Virginia.
Kentucky.	Rhode Island.	Indiana.	West Virginia.
West Virginia.	Connecticut.	Kentucky.	Tennessee.
Tennessee.	Dist. of Columbia.	Illinois.	North Carolina.
Delaware.	Illinois.	Michigan.	South Carolina.
Maryland.	Iowa.	Wisconsin.	Georgia.
Michigan.	Minnesota.	Missouri.	Florida.
Nebraska.	Nevada.	Arkansas.	Alabama.
Montana.	*September.*	Texas.	Mississippi.
Oregon.	Kansas.	Kansas.	Louisiana.
Washington Ter'y.	Missouri.	*July.*	Iowa.
May.	Utah.	New Mexico.	Minnesota.
Virginia.	*December.*	*August.*	Nebraska.
North Carolina.	California.	California.	Nevada.
South Carolina.		*September.*	Utah.
Georgia.		Oregon.	*December.*
Florida.			Delaware.
Alabama.			Vermont.
			Washington Ter'y.

I beg to conclude the somewhat desultory remarks constituting this paper with a tabulated statement of death-rates during the past year in the principal American and foreign cities, derived from official sources. I am largely indebted, for the foreign figures herewith presented, to the courtesy of American consuls at their respective posts. The figures of American, Italian, and British cities, have generally been furnished me by officers in charge of statistical bureaus. This table is believed to be the most comprehensive one of the kind ever presented.

Death-rates in American and Foreign Cities during the Year 1872.

In the cities marked with a star, the population is estimated; in others, actual. It is estimated in all the British cities to the middle of the year 1872.

CITIES IN THE UNITED STATES.

CITY.	STATE.	Population.	Deaths in 1872.	Death-rate per 1,000 Inhabitants.
New York	New York	1,000,000*	32,647	32.6
Philadelphia	Pennsylvania	728,000*	18,987	26.1
Brooklyn	New York	450,000*	12,648	28.1
St. Louis	Missouri	400,000*	8,047	20.1
Chicago	Illinois	367,293	10,156	27.6
Baltimore	Maryland	300,000	7,546	25.1
Boston	Massachusetts	265,000*	8,089	30.5
Cincinnati	Ohio	250,000*	5,116	20.5
New Orleans	Louisiana	200,000*	6,122	30.6
San Francisco	California	188,323	3,232	17.2
Buffalo	New York	150,000*	2,594	17.3
Cleveland	Ohio	120,000*	2,337	19.5
Newark	New Jersey	115,000*	3,636	31.6
Washington	District of Columbia	110,000*	2,230	20.3
Detroit	Michigan	100,000*	2,390	23.9
Albany	New York	95,000*	1,877	19.7
Pittsburg	Pennsylvania	86,076	2,353	27.3
Providence	Rhode Island	72,910	1,610	22.1
Rochester	New York	65,424*	1,188	18.3
Richmond	Virginia	60,000*	1,714	28.6
Memphis	Tennessee	55,000*	2,561	46.6
New Haven	Connecticut	55,000*	1,215	22.1
Alleghany	Pennsylvania	53,180	1,270	23.9
Troy	New York	50,000*	1,702	34.0
Worcester	Massachusetts	47,500*	1,383	29.1
Lowell	"	45,000*	1,046	23.2
Cambridge	"	44,000*	1,068	24.3
St. Paul	Minnesota	40,000*	708	17.7
Fall River	Massachusetts	40,000*	1,067	26.7
Hartford	Connecticut	40,000*	633	15.8
Wilmington	Delaware	37,000*	772	20.9
Portland	Maine	33,000*	760	23.3
Dayton	Ohio	30,473	608	19.9
Lawrence	Massachusetts	30,000*	692	23.1
Quincy	Illinois	30,000*	477	15.9
Evansville	Indiana	30,000*	789	26.3
Lynn	Massachusetts	30,000*	598	19.9
Charlestown	"	28,330	769	27.1
Savannah	Georgia	28,235	1,108	39.2
Elizabeth	New Jersey	27,000*	371	13.7
Peoria	Illinois	26,000*	429	16.5
Salem	Massachusetts	26,000*	602	23.1
New Bedford	"	23,000*	521	22.6
Hoboken	New Jersey	22,000*	723	32.9

Mortality—(continued).

CITY.	STATE OR COUNTRY.	Population.	Deaths in 1872.	Death-rate per 1,000 Inhabitants.
Chelsea	Massachusetts	21,000*	384	18.3
Galveston	Texas	20,000*	559	27.9
Petersburg	Virginia	20,000*	539	26.9
Terre Haute	Indiana	20,000*	365	18.2
Wilmington	North Carolina	18,000*	430	23.9
Sacramento	California	16,298	352	21.6
Gloucester	Massachusetts	16,000*	351	21.9
Denver	Colorado	16,000*	135	8.4
Vicksburg	Mississippi	15,000*	548	36.5
OTHER AMERICAN CITIES.				
Montreal	Canada	121,000*	4,512	37.3
Halifax	Nova Scotia	31,000	961	31.0
Havana	Cuba	200,000*	7,031	35.1
Valparaiso	Chili	100,000*	6,695	66.9
CITIES IN EUROPE.				
London	England	3,311,298	70,893	21.4
Liverpool	"	499,897	13,540	27.1
Manchester	"	352,759	10,079	28.6
Birmingham	"	350,164	8,048	23.0
Leeds	"	266,564	7,425	27.9
Sheffield	"	247,847	6,445	26.0
Bristol	"	186,428	4,110	22.0
Bradford	"	151,720	3,984	26.3
Newcastle-on-Tyne	"	130,764	3,436	26.3
Salford	"	127,923	3,299	25.8
Hull	"	124,976	3,266	26.1
Portsmouth	"	115,455	2,644	22.9
Sunderland	"	100,665	2,672	26.5
Leicester	"	99,143	2,658	26.8
Nottingham	"	88,225	2,235	25.3
Oldham	"	84,004	2,610	31.1
Norwich	"	81,105	2,131	26.3
Wolverhampton	"	69,268	1,795	25.9
Glasgow	Scotland	489,136	13,901	28.4
Edinburgh	"	205,146	5,427	26.5
Dublin	Ireland	310,565	8,970	28.9
Paris	France	1,851,792	39,111	21.1
Lyons	"	323,417	8,645	26.7
Bordeaux	"	194,000	4,648	23.9
Havre	"	86,323	2,712	31.4
Nice	"	52,377	1,670	31.8
Berlin	Germany	828,000*	26,706	32.3
Hamburg	"	338,974	9,049	26.7
Munich	"	169,478	7,077	41.8
Leipsic	"	110,000*	2,553	23.2
Dresden	"	177,055	5,239	29.6

Mortality—(continued).

CITY.	COUNTRY.	Population.	Deaths in 1872.	Death-rate per 1,000 Inhabitants.
Stuttgart	Germany	96,000*	2,226	23.2
Frankfort-on-Main	"	92,000*	1,955	21.2
Bremen	"	85,000*	2,142	25.2
Mayence	"	47,821	1,396	29.1
Vienna	Austria	644,356	20,506	31.8
Prague	Bohemia	162,000*	7,932	48.9
Naples	Italy	448,335	15,996	35.7
Rome	"	244,484	8,977	36.7
Palermo	"	219,398	5,493	25.0
Turin	"	212,644	5,746	27.0
Milan	"	199,009	5,996	30.1
Florence	"	167,096	5,877	35.1
Genoa	"	130,269	4,139	31.8
Venice	"	128,901	3,915	30.4
Bologna	"	115,957	3,736	32.2
Messina	"	111,854	2,700	24.1
Leghorn	"	97,096	2,971	30.6
Amsterdam	Holland	277,766	7,477	26.9
Rotterdam	"	122,471	3,818	31.1
The Hague	"	92,785	2,263	24.4
Copenhagen	Denmark	190,000*	4,487	23.6
Brussels	Belgium	185,000*	4,176	22.6
Antwerp	"	143,545	3,751	26.1
Stockholm	Sweden	140,000*	4,460	31.8
Christiania	Norway	70,000*	1,453	20.7
Cadiz	Spain	54,732	2,445	44.7
Athens	Greece	49,000*	1,621	33.0
Geneva	Switzerland	47,581	923	19.4
Basle	"	46,554	975	20.9
Zurich	"	21,199	294	13.9
CITIES IN INDIA.				
Bombay	India	646,636	18,906	29.2
Calcutta	"	477,600	11,947	25.0
Madras	"	397,552	13,911	35.0
Algiers	Africa	54,874	1,843	33.6

The highest death-rate in the United States, according to this table, was given by Memphis, viz.: 46.6 in each 1,000 inhabitants. The following remarks, however, accompanied these statistics:

"Out of 2,561 deaths, 907 were paupers, nearly all of whom were transient persons passing to and fro on our railroads

and rivers—this city being situated unfortunately in that respect, being a sort of 'catch-all' between North and South of the impoverished, who 'to find work,' etc., change at one period of the year from North to South to escape cold, and at another period from South to North to avoid heat, and for want of employment."

In Savannah the mortality was equal to 39.2 in each 1,000 inhabitants; in Vicksburg, 36.5; in Troy, 34; in Hoboken, 32.9; in New York, 32.6; in Newark, 31.6; in New Orleans, 30.6; and in Boston, 30.5. These were the highest figures of mortality. The other principal cities furnished the following death-rates: Philadelphia, 26.1; Brooklyn, 28.1; St. Louis, 20.1; Chicago, 27.6; Baltimore, 25.1; Cincinnati, 20.5; San Francisco, 17.2. Thus the mortality of New York was apparently high, although its percentage of increase over the previous year was less than that of Philadelphia or Chicago. The exceptional accuracy of our death-returns, and the large number of immigrants and strangers dying within our limits, are special conditions which must be considered as increasing our ratio of mortality far beyond its natural figure.

Of the larger British cities, Dublin yielded the greatest death-rate, viz.: 29.9 in each 1,000 inhabitants; that of Manchester being 28.6; of Glasgow, 28.4; of Leeds, 27.9; and of Liverpool, 27.1. The death-rate of London was as low as 21.4—less than that of any other important British city.

On the Continent of Europe the highest death-rate was noticed in Prague, Bohemia, viz.: the enormous one of 48.9 in each 1,000 people. It was excessive in Cadiz, Spain, where it was equal to 44.7. In Munich it was 41.8; in Rome, 36.7; in Naples, 35.7; in Florence, 35.1; in Athens, 33. In Berlin, a city with little less population than New York, it was 32.3, or nearly equal to our own. In Bologna, Italy, it was 32.2; and in Vienna, Genoa, Stockholm, and Nice, 31.8. The large mortality of the last-mentioned city is owing to the many deaths of invalid strangers sojourning there. High death-rates prevailed also in Havre, Rotterdam, Leghorn, Venice, and Milan, ranging between 31 and 30. In Paris it

was stated as only 21.1, but all deaths by strangers and travelers are there excluded.

The lowest mortality was given by the Swiss cities of Zurich, Geneva, and Basle, 13.9, 19.4, and 20.9, respectively, and Christiania, Norway, 20.7.

Algiers, Africa, gave a death-rate of 33.6. That of the Indian cities of Bombay and Calcutta was by no means high, being 29.2 and 25. In Madras, however, it was 35.

In Montreal it was 37.3; and in Havana, 35.1.

The highest known death-rate prevailed in Valparaiso, Chili, viz., 66.9 in each 1,000 inhabitants. This was the only South American city heard from.

The Academy then adjourned.

STATED MEETING, JUNE 5, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

DRS. W. M. POLK and LOUIS F. SCHULTZE were elected Resident Fellows.

Dr. G. M. SMITH read the following:

CASE OF FACIAL PARALYSIS.

BY GOUVERNEUR M. SMITH, M. D.

MR. PRESIDENT AND FELLOWS OF THE ACADEMY:

THE brief recital of a case of Bell's paralysis which has recently come under my care may prove of interest at this time, as the successful method of treatment adopted tends to show the correctness of views originally presented to this Academy a short time since by one of its distinguished Fellows.

On the 5th of April last a patient came under my care, suffering with paralysis of the left side of the face. The patient was a gentleman of culture and means, about sixty-one years of age. Residing for a large part of the year at his country-seat on the Hudson, and spending much of the

time in the open air, he was ordinarily in the enjoyment of excellent health, and manifested his robust condition by a commanding appearance. The occurrence of such local palsy was the occasion of no little solicitude in the mind of the patient, lest it be precursory of a hemiplegic seizure.

In studying the etiology of the malady it seemed probable that the disorder had been excited by cold, to which the patient had been exposed while riding in the Central Park on the day previous to the one in which the paralysis was fairly developed. There was no evidence of centric disturbance; peripheral lesion was not marked by any decided local point of irritation.

In speaking of peripheral facial hemiplegia, Aitken remarks: "Although it is not a dangerous form of paralysis, it is one from which recovery is very slow, and in which prognosis, as to complete recovery of symmetry of the face, is uncertain;" and also says: "From four to ten months is the ordinary duration of the affection; but there are instances in which the paralysis yields in twenty-four, fifteen, or even twelve hours, but such cases are exceptional" (Trousseau).

After regulating the bowels of my patient, he was placed under the use of iodide of potassium, and on four occasions electrization by a specialist was applied to the affected side. No counter-irritation behind the ear was resorted to, owing to the absence of apparent local lesion. On the 21st of April, the patient had shown little or no improvement. I remembered that Dr. William Detmold had read a paper before this Academy (March 20, 1873), entitled "Facial Paralysis treated by a New Method." Not having been present at the reading of the paper, I called upon Dr. D., and he briefly gave me the views he had here expressed, and as since published in the *New York Medical Journal*, May, 1873.

In the case which he has reported he says: "I determined to try what mechanical means would do. I bent a wire into a hook, which I put into the drooping corner of the mouth, and, drawing it up, bent the wire over and behind the ear. I recommended the patient to keep it on over night, trusting

that, by entirely relaxing the paralyzed muscles, and supporting the dragging weight, I might somewhat relieve the defect." Prompt amelioration followed this method of treatment. Dr. D. further says: "It then occurred to me that I might make this instrument still more effective if I could combine with it a permanent and continuous galvanic current through the paralyzed parts, by having it made of two different metals, thus forming, as it were, a single cell of a galvanic battery." An instrument fulfilling such purpose was made by Mr. Chester, under Dr. D.'s direction, and the patient at the time of the report was steadily improving. The case had been a chronic one of sixteen years' duration, and had not before been relieved. In his conclusion Dr. D. remarks: "I am unable to say what share in the benefit, or whether any, is due to the galvanic current, to which, on the whole, I do not attach as much importance as to the mechanical support."

Resolving to test the applicability of this method of treatment to the acute case under my care, I procured from Mr. Stohmann's a German-silver wire mouth-piece, used by the dentists in holding the mouth open during dental operations. The dentists employ two, one on each side. One of these I bent together so that it would not keep the mouth open, but simply act as a hook comfortably catching the corner of the mouth, and to the outer end fastened a piece of copper wire which, passing across the cheek, was turned around the ear. The wire passing over the ear being covered with a soft material was not a source of irritation. As the cheek was quite pendulous I ordered a bandage to be passed around the head under the jaw, to give further support.

The relief which followed was significant, for, after using this appliance for two nights, decided amelioration was manifest.

Wishing the patient to avail himself of any advantage that might be derived from the galvanic current, I went with him to Mr. Charles T. Chester's, 104 Centre Street, and, giving a wire model as to size, Mr. C. had prepared this neat

instrument, which is a fac-simile in principle of the one made under Dr. D.'s direction. The smooth and easily fitted hook or mouth-piece is of platinum; the wire running across the cheek and turning behind the ear, is of silver, and to this is adapted a zinc plate which is covered with velvet, with the view of readily retaining the moisture of either saline, acidulated, or pure water.

The patient, on procuring this instrument, substituted it for the one I had extemporized—using it at night; convalescence was rapid. Recovery from the facial paralysis was complete in about one month from the time of its incipiency. There has been no recurrence of the difficulty; the symmetry of the face is normal.

Several questions naturally arise in this connection. In the first place, was this case one of those, occasionally met with, in which recovery takes place without material artificial assistance; and in the second place, if recovery was due to treatment, how far was it attributable to the mechanical means, and how far to the galvanism? In response, I would say that there was scarcely any perceptible improvement in the patient, until the "mechanical means" were resorted to; convalescence seemed to date from the night they were employed.

Whether or not the second instrument was a more potent remedial factor by its galvanic properties, it is difficult to say. The patient was not conscious of any galvanic influence, though there is no question of the passage of a current through the affected side, by means of this appliance, but, as stated in Dr. Detmold's paper, from the periphery to the centre. In regard to the action of this instrument, Mr. Chester has written to me as follows: "I have tested the little galvanic battery made to apply to the face of Mr. ——, in a general way. The covering of the zinc plate, being moistened with water, made a good conductor by the addition of a slight trace of acid, and the plate then applied to the ear (behind), while the platinum end was inserted in the mouth, I find that it generates a steady current, capable of deflecting a galvanometer or sending a telegraph-message easily through seventy-five miles of the ordinary telegraph-wire."

It affords me pleasure, Mr. President, to report this case, which, so far as I am aware, is the first acute one treated by the method suggested by the distinguished Fellow to whom allusion has been made.

The surgeons are indebted to Dr. Detmold for originating the operation for strabismus, for his ingenuity in talipes, etc.; we, as physicians, return him thanks for originating a new and successful method of treating a disorder medical in its nature, and will be glad to borrow him from his surgical brothers to aid us further in our department of science.

Dr. J. Lewis Smith then read a paper, of which the following is an abstract:

CEREBRO-SPINAL FEVER WITH FACTS AND STATISTICS OF THE RECENT EPIDEMIC IN NEW YORK.

By J. LEWIS SMITH, M. D.

Mr. President and Fellows of the Academy:

Cerebro-spinal fever is an epidemic constitutional disease, manifesting itself by lesions and symptoms, which pertain chiefly to the nervous system. In exceptional instances during an epidemic, the brunt of the malady falls upon the lungs, pneumonia occurs, and the cerebro-spinal symptoms in these cases appear to be subordinate to the pulmonic. Hence, Dr. Webber, of Boston, in his essay on cerebro-spinal fever, which obtained the Boylston prize, recognizes a form of the disease which he designates pneumonia. Dr. Smith believes that, in the recent New York epidemic, there were occasional cases which presented this form, and he related the following, which occurred in his practice: Mrs. L——, aged about twenty-five years, was seized on June 8th with pain in the head and right subaxillary region. In two or three hours the pain in the thorax ceased, but that in the head continued and was very severe. On the 9th she complained only of the violent frontal headache. On the 10th she stated that objects were very indistinct, having a blurred appearance, but the eyes seemed normal; pulse 118, temperature

105° ; evening, pulse 104, temperature 102°. She moaned, and obtained little or no sleep, on account of the severity of the headache.

13*th.*—The pain in the head ceased, but that in the right side of the chest is more severe, and respiration is accompanied by a moan; respiration 32, pulse 116, temperature 105°. There is rigidity of the muscles of the neck, so that it is impossible to bring forward the head upon the chest. Attempts to do it are painful, and the shoulders move with the head. There are general hyperæsthesia, and well-marked pleuro-pneumonia of the right lower lobe. Death occurred June 17th.

In the above case the violent headache, dimness of sight, marked stiffness of the muscles of the nucha, and the hyperæsthesia, indicated cerebro-spinal fever as the disease, and yet the cerebral symptoms abated in a few days, and the prominent local symptoms in the last days of the sickness were due chiefly to the pneumonia. In another case, which was visited by three prominent physicians of this city, who agreed in the diagnosis of cerebro-spinal fever, pneumonia of the right lung was suddenly developed at about the sixth or seventh day. The chief symptoms subsequently were referable to the pneumonia, and when this abated the patient recovered.

Cause.—Does it emanate from the Soil? The following facts demonstrate that it does not, to wit: most of the epidemics commence in winter when the ground is frozen; the disease occurs in valleys and on hill-tops, and upon all varieties of soil; it invades one district, passes over another adjoining, and affects, perhaps, a third beyond, although the geological formation of all is the same.

Does the cause exist in the diet, as some competent observers have supposed? The following facts, I believe, are sufficient to justify a negative answer: Of two adjacent localities, in which the nature of the diet of the inhabitants is the same, one escapes and the other is visited by the epidemic; an epidemic sometimes prevails here and there over an area of many thousand miles, as recently in North America. It is

hardly reasonable to suppose that any deleterious property would occur in the food over so wide a territory. An epidemic ceases, although the food of the people continues the same. Infants at the breast, having only the mother's milk, are sometimes affected, and likewise certain animals, whose food is very different from that of man, and finally the most careful examinations have hitherto failed to discover any change in the cereals, or other food, or noxious principle, sufficient to explain the occurrence of the disease over a wide extent of territory.

There can, therefore, be little doubt that the cause exists in the atmosphere, though so subtile that we may never be able to detect it. Cerebro-spinal fever is indeed one of many examples in corroboration of the statement made by Humboldt, that there is no subject of scientific inquiry more obscure than the laws which control epidemics. Among the meteorological conditions which favor the occurrence of this disease, cool weather has already been alluded to. Statistics collected in France and the United States show that, while 166 epidemics occurred in the six months commencing with December, only 50 occurred in the remaining six months of the year. According to Prof. Hirsch, whose statistics were obtained largely from Central Europe, there were 57 epidemics in winter or winter and spring, 11 in spring, five between spring and autumn, four commencing in autumn and extending into winter or winter and spring, and six lasting through the entire year.

All observers have remarked the fact that anti-hygienic conditions, though obviously subordinate to the unknown atmospheric cause, nevertheless strongly predispose to this disease. Hence, soldiers in barracks and the poor in tenement-houses suffer most severely. During the recent epidemic in New York, unusually severe or multiple cases occurred for the most part where there were obvious anti-hygienic conditions, as in apartments which were unusually crowded and filthy, or in tenements around which refuse had collected or which had defective drainage. The interesting

chart, prepared under the direction of Dr. Moreau Morris for the Health Board, shows that comparatively few cases occurred in those portions of the city where the sanitary conditions were good. Dr. Smith does not, however, agree with Prof. Hirsch, that the greater crowding, domiciliary and personal uncleanliness, and imperfect ventilation in the cool than in the warm months, explain the fact that epidemics occur chiefly in winter and early spring; for, in clean and well-ventilated apartments in sparsely-settled and salubrious localities, epidemics occur for the most part in these seasons. Anti-hygienic conditions probably predispose to this disease in the same way, and no more than to any other grave epidemic which happens to be prevailing, as for example to Asiatic cholera, whose ravages are largely in the crowded and uncleanly quarters of the poor.

Is Cerebro-spinal Fever propagated by Contagion?—It is the almost unanimous opinion of those who are most competent to judge from their observations, that it is either not contagious, or is so only in a very slight degree. It is certain that the vast majority of cases occur without the possibility of personal communication. Thus, in the commencement of an epidemic, the first patients are affected here and there at a distance from each other, often miles apart, and throughout an epidemic usually only one is seized in a family. Children may be around the bedside of the patient, passing in and out of the room without restriction, and yet we can confidently predict that none of them will contract the disease if there are proper ventilation and cleanliness. And, when two or more cases occur in a family, it commences at such irregular intervals in the different patients that the presumption is strong that they receive it from the same extraneous source, and not one from the other, for contagious diseases usually have a pretty uniform incubative period. Thus, in the Brown family, treated by Dr. Sewall (*Medical Record*, July 1, 1872), the first child sickened January 30th, and the remaining five children at intervals respectively of five, seven, eleven, twenty-five, and forty-five days. The following have been Dr. Smith's observations relating to this point:

Single cases, No. 39 (four adults).

Two in a family, No. 16 (eight families).

Three in a family, No. 3 (one family).

In most of the thirty-nine families in which single cases occurred, there were children who were allowed free intercourse with the patients. Is there any other malady of childhood known to be infectious, which affords such a record of non-contagion? In those instances in which two in a family took the fever, those who were last attacked did not seem to receive it from those who were first affected, for the reason already stated, namely, the very variable intervals between the two cases in the different families. The facts in the family in which three cases occurred did seem to lend support to the doctrine of contagion. A boy twelve years of age died of cerebro-spinal fever, and was buried on Saturday or Sunday. On the following Monday the mother washed the linen of the boy, which had accumulated, and within two days was herself affected with the disease. She and her infant, who was also seized with it, died. Were such cases frequent or not infrequent, the argument in favor of contagion would certainly be strong; but, as they are infrequent, it is proper to accept any other reasonable explanation instead. The state of the bedding and apartments, as observed by the writer, was such as to render the atmosphere in which this family lived noxious in a high degree, and therefore such as to attract the prevailing epidemic. Moreover, the mother, exhausted by her long watching, and deprived of needed sleep (for the boy was several days sick), instead of obtaining the required rest, rendered her system more liable to the fever by her self-imposed duties on the day following the burial. These manifest antihygienic conditions appeared quite sufficient, without the aid of any contagious principle, to explain the occurrence of the cases in this severely-visited family. Dr. Smith's statistics, therefore, harmonize with the doctrine of non-contagiousness, but it is obviously very difficult to determine from clinical experience whether an epidemic constitutional disease is absolutely non-contagious, or contagious in a very low degree.

Cerebro-spinal fever is one or the other, but if contagious it is apparently less so than either typhoid fever or Asiatic cholera.

Allusion has been made to the fact that this malady sometimes occurs among the lower animals. In the epidemic of 1811, in Vermont, Dr. Gallop remarks that even the foxes seemed to be affected, so that they were killed in numbers near the dwellings of the inhabitants. The recent epidemic in New York, it is well known, prevailed among horses several months before it occurred among the people. It was common and fatal in the large stables of the city car and stage lines in 1871, while among the people the epidemic did not properly commence, although there were previously isolated cases, till January, 1872. It has been asked whether, in epidemics like this, in which the lower animals are first affected, the disease may not be communicated from them to man. This obviously brings up the question of contagiousness. Dr. Smith's observations favor a negative answer, for he has not been able to ascertain that those who had charge of the affected horses in the recent epidemic, as the veterinary surgeons or stablemen, were any more liable to the fever than others, who were not so exposed. They apparently were not, and we must, therefore, believe that this disease is not propagated from one species of animals to another, certainly no more than from one animal to another in the same species, and the fact that different animals are affected by the epidemic is due to the potent and pervading nature of the cause. Cerebro-spinal fever is indeed, so to speak, pandemic in a double sense; on the one hand affecting both sexes, different ages, and all conditions of people over a wide extent of territory, and on the other hand different species of animals, but with little or no contagiousness.

Not infrequently we are able to discover some exciting cause of the fever, usually an exhausting or perturbating influence of some sort. An individual, whose system is affected by the epidemic influence, and is therefore predisposed to the disease, may, perhaps, escape by a quiet and regular mode of life; but, if there is an exciting cause of the

nature alluded to, the fever may be developed. Among these exciting causes may be mentioned overwork, fatigue, mental excitement, prolonged abstinence from food, followed by overeating, and the use of indigestible and improper food. Thus, in one instance in Dr. Smith's practice, a delicate young woman, at the head of one of the departments in a well-known Broadway store, was anxious and excited, and her energies overtaxed at th eannual reopening. Within a day or two subsequently the disease commenced. Another patient, a boy, was seized after a day of unusual excitement and exposure, having in the mean time bathed in the Hudson when the weather was quite cool. During the recent epidemic in New York those children seemed especially liable to be attacked who were subjected to the severe discipline of the public schools, returning home fatigued and hungry, and eating heartily at a late hour. In one instance which was observed by Dr. Smith, a school-girl of ten years returned from school excited and crying, because she had failed in her examination and was not promoted. In the evening, after she had closely studied her lessons, the fever commenced with violent headache. Dr. Frothingham (*American Medical Times*, April 30, 1864) writes as follows of the brigade in which cerebro-spinal fever occurred in the Army of the Potomac: "Under General Butterfield, a stern disciplinarian, the men were drilled to the full extent of their powers—often to exhaustion. I did not at the time recognize this as a cause of the disease in question, but I learn that in the present epidemic in Pennsylvania the attack generally follows unusual exertion and exposure to cold." Observers have long recognized the fact of such exciting causes. Dr. Gallop, in his history of the epidemic in Vermont in 1811, directs attention to the severity of the disease among the troops under General Dearborn, who were fatigued by marches, and greatly dispirited by a repulse which they had sustained from the British.

Sex.—It is stated by writers that more males are affected than females. Hospital and military statistics show this; but

in family practice, in which a large proportion of the patients are children, the number of males and females is about equal. Thus in 75 cases occurring in the Twentieth and Twenty-second Wards, mainly in the practice of two other physicians and the writer, there were 39 males and 36 females. Sixty-four of these were children. From January 1 to November 1, 1872, 905 cases, in which the sex was stated, were reported to the Health Board. Of these 484 were males, and 421 females. Dr. Sanderson's statistics of the epidemic in the provinces around the Vistula, the cases being chiefly children, give also but a slight excess of males. Probably, therefore, the sex under the age of puberty makes no difference in the liability to this disease, and the same may be said of all other constitutional affections. Men are more liable than women, only when they lead a more irregular life, and are subject to more privations and exposures.

Age.—Children, as already stated, are much more liable to cerebro-spinal fever than adults. The following are the statistics of the Health Board relating to this point, the cases occurring in 1872:

Under 1 year	125
From 1 to 5 years	336
" 5 " 10 "	204
" 10 " 15 "	106
" 15 " 20 "	54
" 20 " 30 "	79
Over 30 years	71
Total	975

In the statistics of Dr. Smith, which embraced 81 cases, occurring in the Twentieth and Twenty-second Wards, the ages were as follows:

Under 1 year	8
From 1 to 3 years	18
" 3 " 5 "	20
" 5 " 10 "	17
" 10 " 15 "	7
Over 14 years	11
Total	81

It is seen that nearly three-fourths of the whole number of cases in the recent epidemic in New York City were under the age of ten years. The statistics of other epidemics occurring in civil practice is similar. Thus Dr. Sanderson, in examining the mortuary statistics of the epidemic in Germany, ascertained that there had been 218 deaths under the age of fourteen years, and only 17 above that age; and although this does not show the exact ratio of children to adults, in the entire number of cases it is apparent that children greatly preponderated.

The more advanced the age after childhood, the less the liability to this disease; so that after the middle period of life few cases occur, and after the age of fifty years there is nearly an immunity. The oldest two observed by Dr. Smith had attained the ages respectively of forty-seven and sixty-three years.

Symptoms.—Dr. Smith related two cases, which showed abortion of the malady, and one which continued nearly the usual time, but in so mild a form that the patient was able to sit during a considerable part of the sickness. In all the cases which he has observed, and in the records of published cases which he has examined, cerebro-spinal fever began between 12 M. and 6 A. M. The fact that it does not commence after the repose of night till several hours of the day have passed, shows the propriety, as will be seen hereafter, of enjoining a quiet and regular mode of life free from excitement, and with sufficient hours of sleep during the time that the epidemic is prevailing.

Cerebro-spinal fever usually has no premonitory stage, or it is so slight as to escape notice. Exceptionally there are certain premonitions for a few hours or days, such as languor, chilliness, etc. Premonitions occur more frequently in mild than in severe forms of the fever. The ordinary mode of commencement in a typical or somewhat severe case is as follows: The patient has a rigor or chill, or rarely two or three of them at irregular intervals of some hours. One patient, an adult female, had three or four pretty severe chills, the last

occurring, from recollection, as late as the fourth day. Children often have clonic convulsions in place of the chill, or immediately after it, partial or general, slight or severe. Apathy, more or less profound stupor, or less frequently delirium, succeeds. In the gravest cases semi-coma occurs, from which the patient is with difficulty aroused, or profound coma, which, in spite of prompt and appropriate treatment, may prove speedily fatal. If aroused to consciousness, he now complains of violent headache, with or without, or alternating with equally, severe neuralgic pains in the neck, some part of the trunk, or in one of the extremities. The pupils are dilated, or less frequently contracted, and they respond feebly, or not at all, to light. Often they oscillate, and occasionally one is larger than the other.

Vomiting, with little apparent nausea, is also an early and prominent symptom, evidently having a cerebral origin. It occurred as an initial symptom in fifty-one of fifty-six cases observed by Dr. Sanderson. Of sixty-one cases observed by Dr. Sewell and the writer, neither its presence nor absence was recorded in thirteen cases, its absence in only one, and its presence as an early symptom in forty-eight cases.

Unlike typhus and typhoid fevers, the temperature is usually as elevated on the first day as, and sometimes more so than, subsequently. Indeed, the highest temperature which the writer observed in any case was only two or three hours after the commencement of the attack in a child of three years, namely, a temperature of $107\frac{2}{5}°$.

Exceptionally the initial symptoms occur in a more gradual manner, becoming by degrees more severe, so that a few days elapse before they are so pronounced that a clear diagnosis is possible. The febrile movement, headache, neuralgic pains, lassitude, vomiting, and fretfulness, though pretty uniformly present in the commencement, are not in these cases so severe at this period as to excite any apprehension.

Symptoms pertaining to the Nervous System.—Pain, already described as an initial symptom, continues during the acute period of the malady. It is ordinarily severe, eliciting

moans from the sufferer, but its intensity varies in different patients. Its most frequent seat is the head, where it may be frontal or occipital. It is described as sharp, lancinating, or boring. It is also common in the neck, especially the nucha, the epigastrium, umbilical and lumbar regions, in one or more of the limbs, and along the spine (rachialgia). It shifts from place to place, but it is commonly more persistent in the head and along the spine than elsewhere. The patient, if old enough to speak, and not delirious or too stupid, often exclaims, "Oh my head!" from the intensity of his suffering, but after some moments complains equally of pain in some other part, while perhaps the headache has ceased, or is milder. In a few instances the headache is absent, or is slight and transient, while the pain is intense elsewhere. After some days the pains begin to abate, and by the close of the second week they are much less pronounced than previously. Vertigo occurs with the headache, so that the patient reels in attempting to stand or walk. Contributing to the unsteadiness of the muscular movements is a notable loss of strength, which occurs early and increases.

The state of the patient's mind is interesting. It is well expressed in ordinary cases by the term apathy or indifference, and between this and coma on the one hand, and acute delirium on the other, there is every gradation of mental disturbance. Sometimes patients seem totally unconscious of the word or presence of those around them, when it appears subsequently that they understood what was said or done. Delirium is not infrequent, especially in the older children and adults. Its form is various, most frequently quiet or passive, but occasionally maniacal, so that forcible restraint is required. It sometimes resembles intoxication, or hysteria, or it may appear as a simple delusion in regard to certain subjects. Thus, one of Dr. Smith's patients, a boy of five years, appeared for the most part rational, protruding his tongue when requested, and ordinarily answering questions correctly, but he constantly mistook his mother, who was always at his bedside, for another person. Severe active delirium is com-

monly preceded by intense headache. In favorable cases the delirium is usually short, but, in the unfavorable, it is apt to continue with little abatement till coma supervenes.

On account of the pain and disordered state of mind, patients seldom remain quiet in bed, unless they are comatose, or the disease is mild, or so far advanced that muscular movements are difficult from weakness. In severe cases they are ordinarily quiet a few moments, as if slumbering, and then, aroused by the pain, roll or toss from one part of the bed to another. One of the writer's patients, a boy of five years, repeatedly made the entire circuit of the bed during the spells of restlessness. In mild cases patients lie quiet, usually with their eyes closed, except when disturbed.

All writers record a general hyperæsthesia of the skin. Few patients that are not in a state of profound coma are free from it during the first weeks, and it increases materially the suffering. Frictions upon the surface, and even slight pressure with the fingers upon certain parts, extort cries. Gently separating the eyelids for the purpose of inspecting the eyes, and moving the limbs, or changing the position of the head, evidently increase the suffering, and are resisted. Dr. Smith sometimes observed such outcries, from slowly introducing the thermometer into the rectum, that he was forced to believe that the anal, and perhaps rectal, surface was also hyper-sensitive. The hyperæsthesia has diagnostic value, for there is no disease with which cerebro-spinal fever is likely to be confounded in which it is so great. It is due to the spinal meningitis, and is appreciable even in a state of semi-coma.

Tonic contraction of certain muscles, or groups of muscles, is present in all typical cases. In a small proportion of patients it is absent, or is not a prominent symptom, namely, in those in whom the encephalon is mainly involved, the spinal cord and meninges being but slightly affected, or not at all. This contraction is most frequent and marked in the muscles of the nucha, causing retraction of the head, but it is also common in the posterior muscles of the trunk, producing opisthotonos, and in less degree in those of the abdomen and

lower extremities, and hence the flexed position of the thighs and legs, in which patients obtain most relief. The muscular contraction is not an initial symptom. The writer ordinarily first observed it about the close of the second day, but sometimes as early as the close of the first day, and in other instances not till the close of the third day. Attempts to overcome the rigidity, as by bringing forward the head, are very painful, and cause the patient to resist. In young children having a mild form of the fever, with little retraction of the head, the rigidity is sometimes not easily detected. Dr. Smith has been able in these cases to satisfy himself and the friends of its presence, by observing the difficulty with which the head is brought forward on presenting to the patient a tumbler with cold water, which is craved on account of the thirst. The usual position of the patient in bed is with the head thrown back, the thighs and legs flexed, with or without forward arching of the spine. The muscular contraction continues from three to five weeks, more or less, and abates gradually; occasionally it continues much longer. Through the kindness of Dr. Griswold, of Thirtieth Street, the writer was allowed to see an infant of seven months in the tenth week of the disease. It exhibited great fretfulness, decided prominence of the anterior fontanelle, probably from intracranial serous effusion, and marked rigidity of the muscles of the nucha with retraction of the head.

Paralysis occasionally occurs, but is less frequent than we would be led to expect from the nature of the lesions. It may occur early, but it is more frequently a late symptom. It may be limited to one or two of the limbs, as a leg, or arm and leg, or it may be more general. Thus, a man treated by Dr. Law, in the Dublin epidemic of 1865, could move neither arms nor legs, and Wunderlich saw a patient who had paralysis of both lower extremities and a considerable part of the trunk. As the paralysis is due to inflammatory processes in the cerebrospinal axis, it usually disappears in a few weeks as the inflammation abates, and convalescence is established, but it may be more protracted. Thus in Wunderlich's case there was only partial recovery after the lapse of five months.

Digestive System.—The tongue is ordinarily lightly covered with a whitish fur. Occasionally, in cases attended with great prostration, the fur is dry and brown, but only for a few days, when the moist, whitish fur succeeds. The habitual brownish and dry fur on the tongue, and sordes upon the teeth, so common in typhus and typhoid fevers, are seldom observed in uncomplicated cases of this disease. Vomiting which was described as an initial symptom, usually ceases in a few hours, or not till the lapse of several days, and it frequently recurs at intervals during the periods of recrudescence, which are common in the progress of the fever. It occurs with little effort, often like a regurgitation, as is common when this symptom has a cerebral origin. The ejecta consist at first of the contents of the stomach, and afterward partly of bile. It does not differ as a symptom from the vomiting which is so common in sporadic meningitis. Having a similar origin is a sensation of faintness or depression, referred to the epigastrium.

The appetite is poor or entirely lost during the active period of the malady, and it is not fully restored till convalescence is well advanced. On account of the imperfect nutrition, patients progressively waste, and when the case is protracted there is always notable emaciation. Thirst, already alluded to, and more or less constipation, are common, but the latter readily yields to purgatives. On the other hand, diarrhœa sometimes precedes and accompanies the disease. The writer observed this in a few instances in 1872, when the weather had become warm. The patients were young children.

Pulse.—In children this is constantly accelerated, except occasionally in the comatose state. It is liable to daily variations in frequency, which seem sudden and without appreciable cause. In most inflammatory and febrile diseases, exacerbations commonly occur in the latter part of the day, but in this disease they do not seem to be influenced by the time in the day, so that sometimes the pulse is most frequent and temperature highest in the morning, sometimes in the evening,

and then again in the middle of the day. In favorable adult cases the pulse often remains under 100, and in certain patients it scarcely has more than normal frequency; but if the type is severe it rises to 110, 120, or upward.

Temperature.—The internal temperature is always above the normal standard during the active period of the disease. The highest temperature observed in about fifty cases was $107\frac{2}{5}°$, which was on the first day, and the lowest $99\frac{1}{2}°$. There are greater and more sudden variations in this symptom than occur in any other inflammatory and febrile disease.

Respiratory System.—There are usually no notable symptoms pertaining to the respiratory system, except when the disease is complicated with pulmonary congestion, inflammation, or œdema. Intermittent, sighing, or irregular respiration is less frequent in cerebro-spinal fever than in sporadic meningitis; but it does occur. In thirty-one observations in children who had the disease without complication, the average respirations were forty-two per minute, while the average pulse was 137. The fact that the respiration as compared with the pulse was proportionately somewhat more frequent than in health, was probably due to tonic contraction, more or less pronounced, of certain of the muscles concerned in the respiratory movements.

Cutaneous Surface.—The features may be pallid, of normal appearance, or flushed in the first days of the disease; but in advanced cases they are pallid, as is the skin generally. A circumscribed patch of deep congestion often appears, as in sporadic meningitis, upon some parts of them, as the cheek, forehead, and ear, and after a short time disappears. Friction for a moment upon any part of the surface, when the temperature is not reduced, produces the same appearance—a fact to which Trousseau and others have called attention as regards sporadic meningitis.

The following are the abnormal appearances of the skin which Dr. Smith has most frequently observed: 1. Papilliform elevations, due to contraction of the muscular fibres of the corium, namely, the so-called goose-skin. This is not

uncommon in the first weeks. 2. A dusky mottling, also common in the first and second weeks, in grave cases, and most marked where the temperature is reduced. 3. Numerous minute red points over a large part of the surface, bluish spots, a few lines in diameter, due to extravasation of blood under the cuticle, resembling bruises in appearance, and large patches of the same color, an inch or more in diameter, less common than the others, and usually not more than two or three upon a patient. These last the writer believes, from certain observations, are sometimes the result of bruises which the patients receive during the spells of restlessness. 4. Herpes. This is common. It sometimes occurs as early as the second or third day, but in other instances not till toward the close of the first week or in the second. The number of herpetic eruptions varies from six or eight to a dozen or more. This affection evidently has a neuropathic origin, the vesicles occurring chiefly on those parts of the surface which are supplied by branches of the fifth pair of nerves. Its most common seat is upon the lips, but the writer has occasionally observed it upon the mucous membrane of the nasal and buccal surfaces, upon the cheek, around the ears, and upon the scalp.

During the first days the skin is apt to be dry. Afterward perspirations are not unusual, and free perspirations sometimes occur, especially about the head, face, and neck. The quantity of urine excreted is normal, or it may be in excess of the normal amount. It occasionally contains a moderate amount of albumen, and in exceptional instances cylindrical casts and blood-corpuscles. A deposit of urates in the urine is not infrequent, but this so often occurs in inflammatory and febrile diseases that it is of little moment.

Arthritic inflammation, apparently of a rheumatic character, has been occasionally observed in most epidemics. It is commonly slight, producing merely an œdematous appearance around one or more joints. Thus, in one case, which came under the writer's notice, and which was subsequently fatal, the parents, who were poor, and were therefore without medical advice till the case was somewhat advanced, had already

diagnosticated rheumatism on account of puffiness, which they had noticed around one of the wrists.

The organs of the special senses are more or less involved in most cases, and the eye and ear are not infrequently the seat of serious lesions. Taste and smell are rarely affected, so far as known, but it is possible that they may sometimes be perverted or even temporarily lost during the time of greatest stupor. In one case at least the smell in one nostril was entirely lost. The affections of the eye and ear are the most important and interesting of those of the special senses. Strabismus is common. It may occur at any period of the fever, continuing a few hours or several days, and it may appear and disappear several times before convalescence is established. Occasionally it continues several weeks, but with few exceptions the parallelism of the eyes is finally restored. In a boy of five years, whom Dr. Smith saw the last three months after convalescence, there were still convergent strabismus of the right eye and double vision.

Changes in the pupils are among the first and most noticeable of the initial symptoms, as I have already stated in describing the mode of commencement. These are dilatation, less frequently contraction, oscillation, inequality of size, feeble response to light, etc. Most patients present one or more of these abnormalities of the pupils, and they continue during the first and second weeks, and gradually abate, as the condition of the patient improves. Inflammatory hyperæmia of the conjunctiva often occurs. It commences early, and now and then the conjunctivitis is so intense that considerable tumefaction of the lids occurs, with a free muco-purulent secretion. The false diagnosis has indeed been made of purulent ophthalmia, in cases in which this affection of the lids was early and severe. But such intense inflammation is quite exceptional. More frequently, there is a uniform diffused redness of the conjunctiva, not so dusky as in typhus, and the injected vessels cannot be so readily distinguished as in that disease.

In certain cases, almost the whole eye (all, indeed, of the

important constituents) becomes inflamed; the media grow cloudy, the iris discolored, and the pupils uneven, and filled up with fibrinous exudation. The deep structures of the eye cannot, therefore, be readily explored by the ophthalmoscope, but they are observed to be adherent to each other, and covered by inflammatory exudation. They present a dusky-red, or even a dark color, when the inflammation is recent. Exceptionally, the cornea ulcerates, and the eye bursts, with a loss of more or less of the liquids, and shrinking of the eye. But ordinarily no ulceration occurs, and, as the patient convalesces, the œdema of the lids, hyperæmia of the conjunctiva, the cloudiness of the cornea, and of the humors, gradually abate, and the exudation in the pupils is absorbed. The iris bulges forward, and the deep tissues of the eye, viewed through the vitreous humor, which before had a dusky-red color from hyperæmia, now present a dull-white color. The lens itself, at first transparent, after a while becomes cataractous. Sight is lost, totally and forever. This form of ophthalmia is sometimes rapidly developed, as in the following example:

On July 5, 1873, the writer was called to a boy, five years of age, who had reached the tenth day of cerebro-spinal fever without apparently any affection of the eyes, as both presented the normal appearance. On the following day the left eye was red and swollen from the inflammation and chemosis, so that the lids could not be closed, and the media were cloudy. Death occurred on the same day.

If the patient live, the volume of the eye diminishes, as the inflammation abates to less than the normal size, even when there has been no rupture, and divergent strabismus is apt to occur. Prof. Knapp says: "The nature of the eye-affection is a purulent choroiditis, probably metastatic." Fortunately, so general and destructive an inflammation of the eye, as has been described above, is comparatively rare. On the other hand, conjunctivitis of greater or less severity, and hyperæmia of the optic disk, consequent on the brain-disease, are not unusual, but they subside, leaving the function of the organ unimpaired.

Inflammation of the middle ear, of a mild grade and subsiding without impairment of hearing, is common. The membrana tympani, during its continuance, presents a dull-yellowish, and in places a reddish hue. Occasionally a more severe otitis media occurs, ending in suppuration, perforation of the membrana tympani, and otorrhœa, which ceases after a variable time. But otitis media is not the most severe affection of the sense of hearing. Certain patients lose their hearing entirely and never regain it, and that too with little otalgia, otorrhœa, or other local symptoms, by which so grave a result can be prognosticated. This loss of hearing does not occur at the same period of the disease in all cases. Some of those who become deaf are able to hear as they emerge from the stupor of the disease, but lose this function during convalescence, while the majority are observed to be deaf as soon as the stupor abates and full consciousness returns.

Two important facts have been observed in reference to the loss of hearing in these patients, namely, it is bilateral and complete. When first observed it is sometimes complete, but in other instances it is partial, and when partial it gradually increases till after some days or weeks, when it becomes complete. Dr. Smith has the records of ten cases of this loss of hearing, or about one in ten of the total number of cases which have either come under my observation or have been reported to me by physicians in whose practice they occurred. One was a young lady, and the others children under the age of ten years. Prof. Knapp has examined thirty-one cases. "In all," says he, "the deafness is bilateral, and, with two exceptions of faint perception of sound, complete. Among the twenty-nine cases of total deafness there was only one who seemed to give some evidence of hearing afterward."

One theory attributes the loss of hearing to inflammatory lesions, either at the centre of audition within the brain, or in the course of the auditory nerves before they enter the auditory foramina. Thus Stillé says: "This symptom appears to depend chiefly upon the pressure of the plastic exudation in which the nerves are embedded." The other theory attributes

the loss of hearing to inflammatory disease of the ear, and especially of the labyrinth. Dr. Sanderson, who is an advocate of this latter theory, remarks as follows: "As regards the nature of the affection, there appears to be good reason for believing that, like the blindness observed under similar circumstances, and sometimes in the same cases, it is dependent on inflammatory changes in the organ of hearing itself. Dr. Klebs was kind enough to show me in the pathological museum of the Charité, at Berlin, a preparation of the internal ear of a soldier who had died of epidemic meningitis complicated with deafness, in which fibrinous adhesions existed between the bones of the internal ear and the walls of the vestibule. Dr. Klebs stated that in the recent state the mucous lining of the vestibule was detached." In the case of a young woman who was deaf from the commencement, and died on the eighth day, "both tympana were natural, but in the left membrana tympani was found a dense white thickening as large as a pin's-head. On the same side the lining membrane of the semicircular canals was distinctly thickened and loosened, and in the anterior canal there were semi-fluid purulent masses." Prof. Knapp also states: "The nature of the ear-disease is, in all probability, a purulent inflammation of the labyrinth." According to him no disease of the middle ear could cause such complete deafness, and, as evidence that the deafness is not due to central disease, Dr. Gruening obtained by electrization the normal reaction of the auditory nerve within the cranium. Moreover, if the lesion which destroys hearing is within the cranium, why is not the function of the other cranial nerves also abolished? Drs. Keller and Lucae have also, in three *post-mortem* examinations, found evidences of disease of the labyrinth.

An argument in support of the former of these theories is the fact that the lesion which produces the deafness is not ordinarily attended by any marked subjective symptoms referable to the ear, as otalgia, etc. Again, the fact that the deafness is always bilateral, and simultaneous in the two ears, comports better with the doctrine of a central lesion than with

that which locates the lesion in the ear. But the true theory can only be positively established by dissections, and, as we have seen, several *post-mortem* examinations have revealed inflammatory disease of the labyrinth in those who have died having this form of deafness, while in no case, so far as I am aware, has the ear been found free from inflammatory lesions. Therefore, the theory which ascribes the deafness to disease of the ear is much better established than the other, and in the present state of our knowledge we must accept it. Moreover, most of the aurists of this city, who have had excellent opportunities to examine these cases, believe in this theory.

Treatment.—In the opinion of the writer, bromide of potassium should be employed internally in the first days of the disease as a nervous sedative, and in order to diminish the active congestion of the cerebro-spinal axis. Abstraction of blood by leeches or otherwise is in most instances improper. General sustaining measures are required, and after the bromide is discontinued tonics are useful. A small dose of morphine or other opiate relieves the pain effectually and safely. The head and nucha should be covered with a bladder or bag of ice till the active period of the fever has passed, and counter-irritation should be employed over the nucha, and along the spine.

On the conclusion of the reading of this paper—

Dr. D. B. St. John Roosa said that, in common with the other Fellows of the Academy, he would have preferred to hear the whole of Dr. Smith's valuable paper, so much of which had been omitted, than to make any remarks himself. The *post-mortem* examinations of the ears of persons who had become deaf from cerebro-spinal meningitis were as yet very few in number, and we could not say, with any certainty or even with any great probability, what was the lesion in the ear, or just where it was situated. Some few years ago Voltolini, of Breslau, asserted that an inflammation of the labyrinth was sometimes mistaken for cerebro-spinal meningitis. The speaker, like other physicians not in general practice, had few opportunities to see cases of aural disease occurring in the

course of "spotted fever," until the constitutional disease had run its course; but he was inclined to believe that Voltolini was correct, and that there was a disease of the internal ear which was occasionally mistaken for cerebro-spinal meningitis.

The Academy then adjourned.

STATED MEETING, JUNE 19, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

THE SECRETARY announced the death of Dr. S. CONANT FOSTER, formerly Recording Secretary and Vice-President of the Academy, at Nassau, N. P., on the 18th of April last, in the fifty-sixth year of his age.

Dr. R. J. O'SULLIVAN then read a paper on "The Hygiene of the Primary Schools," which was discussed by Drs. AGNEW, JACOBI, and PETERS. (Published elsewhere.)

The Academy then adjourned to September 18, 1873.

STATED MEETING, SEPTEMBER 18, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

THE PRESIDENT announced the death of Dr. R. CRESSON STILES, aged forty-two years, on April 19th; HENRY S. HEWIT, aged forty-seven years, on August 19th, and J. H. VANDERVEER, aged fifty-five years, on August 20, 1873, Fellows of the Academy. He also announced the receipt of the following named books, as contributions to the library; "The Medical and Surgical History of the Rebellion," Vol. I., Parts 1 and 2, from the Surgeon-General's Office; "Report of the Columbia Hospital for Women, at Washington, D. C.," from Dr. J. H. THOMPSON; The *Edinburgh Medical and Surgical Journal* for July and August, 1873; and six pamphlets on surgical subjects, from Dr. F. DICHIARA, of Palermo, Italy.

Prof. S. M. MOORE, of Rochester, N. Y. (by invitation), read a paper on "Epiphyseal Fracture at the Superior End of the Os Humeri."

The Academy then adjourned.

STATED MEETING, OCTOBER 2, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

THE PRESIDENT announced the receipt of "Circulars of Information of Bureau of Education," Nos. 1, 2, and 3, as a contribution to the library.

Drs. HENRY D. NICOLL and SAMUEL H. ORTON were elected Resident Fellows.

Dr. J. G. ADAMS, Corresponding Secretary, announced the death of Dr. AUGUSTE NÉLATON, of Paris, France, a Corresponding Fellow of the Academy.

Dr. JOHN O. STONE read a memoir of the late Dr. SAMUEL CONANT FOSTER, formerly Recording Secretary and Vice-President of the Academy.

Dr. L. M. YALE then read a paper entitled "The Antipyritic Treatment of Fever."

The Academy then adjourned.

STATED MEETING, OCTOBER 16, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

THE PRESIDENT announced the receipt of the *Edinburgh Medical Journal* for September, and the first seven numbers of the *Sanitarian*, presented by the author, Dr. A. N. BELL.

Dr. J. G. ADAMS, Corresponding Secretary, read a memoir of the late Dr. AUGUSTE NÉLATON.

Dr. W. H. THOMSON then read a paper entitled "The Management of the Alimentary Canal in Disease."

Dr. O'SULLIVAN, in behalf of the special committee appointed, read resolutions in memory of the late Dr. H. S. HEWIT.

The Academy then adjourned.

STATED MEETING, NOVEMBER 6, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

DR. EDWARD C. SEGUIN was elected a Resident Fellow.

Dr. ADAMS, Corresponding Secretary, announced the death

of Sir Henry Holland, of London, England, a Corresponding Fellow of the Academy.

The Secretary announced the death of Dr. Samuel Blois, a Resident Fellow of the Academy.

Dr. Austin Flint, Jr., then addressed the Academy on the subject of "Cholestinæmia."

Dr. C. A. Leale read the following paper:

ON INTRA-RECTO-ABDOMINAL EXPLORATION.

By CHARLES A. LEALE, M. D.

Mr. President and Gentlemen of the Academy:

We can easily imagine how the examination of the abdomen, by passing the hand through the rectum, will prove a valuable means of diagnosticating certain pathological conditions of the uterus, ovaries, bladder, intestines, liver, kidneys, and sometimes the stomach and spleen; as the following history shows:

While the patient was under the influence of an anæsthetic, my hand was readily passed through the anus, thence along the rectum, and beyond the sigmoid flexure of the colon to its descending portion, after which it was easily pressed from one side of the abdomen to the other; then superiorly to a distance four inches above the umbilicus, where the finger-points were distinctly felt, and counted through the abdominal parietes, when Dr. Sears pressed with his hand from the outside against them. The mode of proceeding was as follows: While the patient was lying on her back, with head very low, to accelerate the cerebral circulation, I covered my hand and entire forearm with lard; then, with fingers held cone-shaped, the hand was slowly, and with great care, passed through the anus, with much less difficulty than is usual when the hand is passed, *post partum*, into the uterus. After passing the entire hand beyond the sphincter ani, a minute's pause was made, then the hand was gradually passed gently along the rectum, until the curve and constrictions of the sigmoid flexure were

reached, when another moment's pause was made while the finger-points were approximated cone-like; the hand was then slowly, gradually, and with great care, pressed forward, then the fingers were separated, when the constrictions were easily overcome, and the hand pushed through the part dilated: digital dilatation was thus continued, until the hand had passed into the descending colon — the sensation imparted while overcoming the constrictions exactly resembling that produced when a partially-spoiled silk elastic is being drawn out, or, to use an exact mode of communication, I should say, one-tenth tearing to nine-tenths stretching. With the hand in the descending colon, and the finger-points sixteen inches beyond the anus, I found that the hand could be passed from side to side, and up and down, within the abdominal cavity, to the right side as far as the liver, and superiorly to a distance four inches above the umbilicus, at which point Dr. H. Thatcher Sears felt and counted the different points of my fingers, as two or more were extended, to enable him to confirm what I had previously stated, when I pressed the hand in the abdominal cavity against my other hand held outside on the surface, near the stomach. The hand, while being withdrawn, was made to explore the surroundings; the return through the sigmoid flexure was without resistance, and the hand was again in the rectum, when the wonderful ease by which uterine or ovarian conditions could be diagnosticated with such certainty was clearly proved. As the woman had dysmenorrhœa, this gave me an opportunity to seek for its cause. With great ease the fundus uteri was grasped in the palm of the hand, and examined—which could be done, by gliding the fingers over it, with nearly as much ease as if it were examined from an opening above the pubes.

The uterus was retroverted, and much hypertrophied, and, although she had not borne children for over ten years, was about five inches wide. At one portion quite a prominence was felt, which I diagnosticated as an interstitial fibroid, size of an English walnut, about four inches in circumference. The hand was then passed from one ovary to the other, then

withdrawn; not having the slightest sign of blood nor emitting any odor of chloroform.

I should state that, before the woman had attempted suicide, she had thoroughly evacuated her bowels, which necessarily greatly expedited the passage of the hand.

She is very large, weighing about two hundred pounds; has a fat pendulous abdomen, measuring forty-nine and a half inches in circumference at the umbilicus. My hand had been introduced sixteen inches, and the greatest dilatation of the anus was ten inches, corresponding to the largest circumference of the forearm.

Prof. Frank H. Hamilton, in the second edition of his work on Surgery, states: "It is possible, indeed, to introduce the whole hand into the rectum, and remove whatever it may contain, as my pupils at Bellevue saw me do during the winters of 1869–'70."

Prof. William H. Van Buren, in his lectures on "Diseases of the Rectum," 1870, states: "We have a means at our command for thorough exploration of the rectum, with a patient under the full influence of an anæsthetic. . . . On a table of proper height and in a good light, the trunk of the body in the prone position, with outspread arms, and the hips properly elevated, so that the intestines gravitate toward the diaphragm . . . I have often, with the aid of Sims's speculum vaginæ alone, obtained an excellent view of the whole internal surface of the rectum as high up as its termination in the sigmoid flexure of the colon."

In an article on the "Reduction of the Womb," in the American translation of Velpeau's "Operative Surgery," by Dr. Mott, 1847, vol. iii., p. 798, after speaking of the different methods, it states: "If all these resources fail, we may, should it be practicable, imitate Dusaussoy, who, introducing the entire hand into the intestines, was enabled to overcome difficulties which no other means could remove but that."

Prof. Fordyce Barker, in 1856, was called in consultation by Dr. Hosack to see a woman between three and four months pregnant, whose bladder had become very much distended

with urine in consequence of a retroverted uterus pressing against the neck of the bladder. Several had in vain made attempts to relieve her by passing a catheter; this Dr. Barker also tried to do as the others had done, but, finding it impossible, he introduced his entire hand through the anus, pushed the uterus upward and forward, then was easily enabled to introduce the catheter, and empty the bladder. After this replacement of the uterus, no further trouble followed; the woman went her full term, and was delivered of a healthy child.

In 1858 Dr. Barker resorted to the same procedure. In this instance he was requested by Dr. Duggan to see a lady advanced to the fourth month of her pregnancy, but who, in consequence of a retroverted uterus, was threatened with an abortion. By means of the hand in the rectum, he replaced the uterus, thereby relieving distressing symptoms, when the woman went nine months and gave birth to a large child. Again in 1869, in consultation with Dr. John E. Stillwell, Dr. Barker replaced a retroverted uterus, which was the cause of intense pain at the fourth month of pregnancy, with the same good results.

In surgical literature we have abundant evidence that digital and sometimes manual examination of the terminal portion of the rectum has been for many years a recognized means of diagnosticating diseases near the anus, but to Prof. G. Simon, I believe, is due the honor of making a great advance, and reaching the sigmoid flexure, for the purpose of exploring the lower portion of the abdominal cavity. (*See American Journal of the Medical Sciences* for July, 1873.) To do as Prof. Simon had done, and even more, to pass my hand beyond the sigmoid flexure, was found to be a very easy procedure, in the case narrated, where we had a fat, pendulous abdomen, such a one, if necessity required, as would present the most difficult obstacles for an "extra-abdominal manual exploration." In the London *Lancet* translation, Dr. G. Simon is made to state: "It is possible to introduce the whole hand into the rectum, without producing any serious lesion. Dila-

tation may be performed with or without previous section of the sphincters. The patient being under the influence of chloroform, two fingers are gently introduced, then three, then the whole hand, and even the forearm. After the hand has reached the sacro-vertebral angle, the fingers may be extended into the abdominal region, and explore the situation from the kidneys to the umbilicus. In this manner affections of the uterus, the ovaries, and even the stomach and spleen, may be diganosed with greater certainty. On introducing only one-half of the hand, the base of the uterus and the ovaries may be explored. In men the fundus of the bladder can be examined, and the existence of calculi detected. In two cases of ovarian cysts, Dr. Simon was enabled by his procedure to make out the length and thickening of the pedicle, the absence of adhesion to the walls of the pelvis, and the existence of fibrous tumors in the fundus of the uterus. The diagnosis was confirmed by the operator."

In a lecture on the "Displacements of the Uterus," published in the *American Medical Monthly, and the New York Review*, August, 1860, Prof. E. R. Peaslee stated, "It is a fact of great practical importance that the whole hand even may be passed into the rectum of the male or female adult." The late Mr. Thomas, of London, first ascertained this fact in respect to the male. Mr. Thomas had a patient, who, in consequence of habitual constipation, had, by means of pushing a piece of his walking-cane along the rectum, been enabled to have a movement from the bowels. One day, having pushed the portion of cane beyond the anus, it was lost, and was removed by Mr. Thomas, after introducing his hand into the rectum, when it was easily seized and withdrawn. Dr. Parent also succeeded in replacing the uterus, only after he had passed the whole hand into the rectum. "And this is in any case to be attempted, if the reduction cannot otherwise be effected." At the last meeting of the Obstetrical Section of this Academy, Prof. Peaslee gave three illustrative cases, where, in each, abortion at four months was imminent, but was prevented by anæsthetizing the patient with ether, then

passing the hand into the rectum, and replacing a retroverted uterus, after which the pregnancy advanced to full term.

History of my illustrative cases:

Case I.—Mrs. H., aged forty-five years, the grandmother of several children, at 9½ P. M., August 28, 1873, while suffering intensely from an attack of dysmenorrhœa, causing temporary aberration of mind, drank, with suicidal intent, two ounces, by weight, of pure chloroform. Half an hour after she had swallowed it, I was at her bedside, and found that none had been vomited. She was lying on the bed, profoundly anæsthetized, surrounded by her friends, who, in vain, were endeavoring to arouse her. The bottle, labeled chloroform, with a mark showing the exact quantity it contained, and which she had taken, was handed to me. I immediately tried to restore her, by briskly slapping the cheeks and arms, and to produce emesis, with the only convenient article, viz., salt-and-water. About half a drachm of the former was placed on the back of her tongue, over which was poured two ounces of cold water. This caused sufficient irritation of the fauces to make her swallow what the mouth contained, and almost immediately to eject the contents of the stomach, consisting of about half an ounce of bread in the form of an emulsion, thereby proving that the stomach was nearly empty when the poison had been taken. The *ejecta* did not emit the slightest odor of chloroform, while, at the same time, the expirations were strongly charged with its vapor, consequently demonstrating that the entire two ounces had been absorbed into the general circulation, and was being rapidly eliminated from the blood by the lungs. I then washed out the stomach by means of a pump; then injected about a pint of water at 100° Fahr. for the purpose of causing it to remove effete material by acting as a brisk diuretic.

At 10.35 the pulse was 72, respiration 48, temperature of skin normal, pupils moderately dilated, not in the slightest degree sensible to the action of a bright light when a candle was passed in front of them; nor was there the least resistance when the conjunctiva was touched.

At 10.40 pulse 80, respirations 32. At 10.47 pulse 104, respirations 40; touching conjunctiva, or pressing the point of a knife into the sole of foot and skin elsewhere, did not produce the faintest sign of reflex action. At 11.02 the pulse was 140, respiration 48, rectal temperature 97° Fahr.; heart's action becoming distinctly more feeble, surface of body cyanotic, very cold, and covered with beads of perspiration, evidently death impending by asthenia. While withdrawing the thermometer from the rectum, I found the sphincter-ani muscles so completely relaxed, that it easily enabled me to guardedly employ direct irritation of the solar plexus of the great sympathetic nerve, and thereby almost as directly one of its plexuses, the phrenic or diaphragmatic plexus. This was done by passing the hand into the descending colon, then above the umbilicus, when it was moved guardedly from side to side in the abdominal cavity, thereby pressing and directly irritating the solar plexus of nerves, which simultaneously stimulated the respiratory act, soon followed by the first sign of returning consciousness.

To prevent the continued abnormal lowering of temperature, artificial heat was constantly applied to the surface of the body. At 11.45 pulse 120, respirations 40; moves feet when pricked with point of knife. At 11.52 pulse 100, respirations 32, resists for the first time the vapor of liquor ammoniæ *fortissim.*, which produced a slight spasm of the glottis when held at nose. At 12.30 body was wrapped in a very hot wet blanket. At 1 A. M., very slowly improving. At 2 A. M., improving; does not answer when spoken to, but moves when severely pinched. She was placed in a comfortable position, and artificial heat continuously applied, and was now sufficiently restored to convince us that Nature would finish the convalescence that art had commenced.

7 A. M.—Found her with slight febrile movement, had severe headache, but promising a complete and speedy recovery, demonstrating that the irritation of the diaphragmatic plexus of nerves, and the prevention of that abnormal fall in the temperature of the blood which is seen in most case of nar-

cotic poisoning, were the means that prevented death; also proving the great dilatability of the anus and elasticity of the intestines, enabling the manipulator to pass the fingers over the fundus and anterior surface of the uterus, to explore over a much broader and higher surface of the abdomen than we should expect by any manipulation when made *post mortem*, when *rigor mortis* had once existed, and pointing to the great clinical value of the use of the thermometer in cases where the prolonged action of an anæsthetic is required, or in cases of poisoning by chloroform; and last, but not least, the great value of intra-recto-abdominal manual explorations.

Three days after having taken the chloroform, she was working the sewing-machine. On examination, I found the sphincter ani normal; that she had been able to retain her fæces, having had very little pain with first movement from the bowels; that there had not been gastritis, the only unpleasant symptom being a headache. No trouble with intestines, and in a few days complete recovery followed. The suggestion to directly irritate the solar plexus of nerves was prompted by the remembrance of the history of another instance where chloroform had been taken to cause suicide, and where, by the judicious application of electricity just above the umbilicus for eight hours, our patient was saved.

Case II.—On the midnight of November 15, 1869, Dr. E. C. Harwood sent for me to see with him a man aged twenty-eight years, who in vain had desired a wealthy young lady to give her consent to become his wife. After repeated proposals on his part, and procrastination on hers, at 10 P. M., in the parlor, while surrounded by several friends, he got down on his knees in front of her, begged that she would procrastinate no longer; and, on her refusal, he withdrew a bottle containing one ounce of pure chloroform from his pocket, the entire contents of which he drank, at the same time, saying that he would die at her feet. About 12 P. M., when I saw him, he was profoundly anæsthetized. Dr. Harwood and myself, by means of artificially inducing respiration, and the frequent application of electricity, succeeded at 6 A. M. in restoring him sufficiently

to enable us to hope for the recovery which followed, after an alarming attack of pharyngitis and gastritis. In this instance we noticed the good effect of electricity when applied over the solar plexus of the sympathetic nerve, by increasing the force of the inspiration. But it required twice the length of time in the instance last narrated, when only one ounce of chloroform had been taken, and when electricity was applied to the pneumogastric, and to the diaphragmatic plexus from the outer surface, that it did in the first recorded case, where two ounces had been swallowed, and where resuscitation was produced by guardedly irritating the solar plexus of the great sympathetic nerve, by means of intra-recto-abdominal manipulation.

The Academy then adjourned.

STATED MEETING — ANNIVERSARY — NOVEMBER 20, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

DRS. HENRY D. NICOLL, S. H. ORTON, WM. M. POLK, LOUIS F. SCHULTZE, and E. G. SEGUIN, were inaugurated Resident Fellows of the Academy.

Prof. JOHN C. DALTON then delivered the Anniversary Discourse, his theme being "The Origin and Propagation of Disease." Published.

The Academy then adjourned.

STATED MEETING, DECEMBER 4, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

DRS. EDWIN F. WARD and JULIUS PARIGOT were elected Resident Fellows.

The Annual Reports were read, and officers for the ensuing year nominated.

The PRESIDENT announced the receipt of the "Proceedings of the Royal Medical and Chirurgical Society of London," Vol. VII., No. 4, *Edinburgh Medical Journal* for November, 1873,

"Circular of Information of the Bureau of Education," No. 4; a pamphlet "On Excision of the Thyroid Gland," by P. H. Watson, M. D., of Edinburgh; "A Treatise on Aphasia," by Frederick Bateman, M. D., presented by the author, as contributions to the library.

The Secretary announced the death of Drs. P. B. Guernsey and M. W. Williams, Resident Fellows of the Academy.

Dr. F. P. Foster then read the paper of the evening, entitled:

PROPOSITIONS IN REGARD TO ANIMAL VACCINATION.

By FRANK P. FOSTER, M. D.,
DIRECTOR OF VACCINE DEPARTMENT OF THE NEW YORK DISPENSARY.

Mr. President and Fellows of the Academy:

I have not prepared a formal paper, for the reason that, although I have never presented this subject before any of the medical societies of New York, the majority of the gentlemen present are, I think, acquainted, at least in outline, with what opportunity has enabled me to observe in regard to animal vaccination during the last three years. I have, however, written down certain propositions, which seem to me to embody the truth concerning the principal points which have been, and perhaps now are, to a certain extent, the subjects of difference of opinion; and, in order not to take up the time of the Academy needlessly, I shall content myself with stating, orally, certain facts which go to support them.

Proposition I.—*The vaccination of the human subject with animal vaccine is attended with a degree of success, as regards the production of vaccinia, equal, to say the least, to that obtained by any other method of vaccination.*

I use the term "any other method" advisedly, meaning not merely that stored animal vaccine furnishes a degree of success equal to that obtained with stored humanized virus, but that, as I hope to show you, this degree of success is not exceeded even by *arm-to-arm* vaccination, i. e., the direct transfer of fluid lymph from the arm of one child to that of another.

For the sake of comparison, we must first establish a standard of the degree of success which may fairly be expected to be attained, under ordinarily favorable circumstances, by the arm-to-arm method. We are not called upon to accept, as this standard, that phenomenal degree of success which has sometimes been reached by vaccinators of exceptional skill, such, for instance, as Mr. Marson, who, as stated by Seaton ("Hand-Book of Vaccination," London, 1868, p. 160), once did over 2,000 vaccinations without a single failure. This degree of success, I repeat, we are not to accept as the standard, although some of the results which I shall call your attention to are even more remarkable.

I have thought it fair to take, as the standard, the results obtained by the vaccinators of London. I presume it will be generally admitted that arm-to-arm vaccination is as carefully and skillfully done in Great Britain as anywhere, and the public vaccinations of London must certainly be a fair example of those of Great Britain at large. I have here Mr. John Simon's well-known and deservedly-esteemed "Blue-book," entitled "Papers Relating to the History and Practice of Vaccination," published in 1857. On page 72 of the work I find a tabular statement of the "infantine vaccinations performed in the metropolis during the triennial period 1854–'56," showing, in separate columns, the number of births, the number of successful vaccinations of children under one year of age, the total number of such vaccinations, and the proportion of vaccinations to births—as they occurred in the various parishes composing the British metropolis. I have calculated, in the case of each parish, the proportion of the successful vaccinations to the whole number, and have added the results in a fifth column. In three of the parishes there were no failures, the percentage of successes being, as a matter of course, 100; in one parish the percentage is recorded as low as 85.65; and, in the remaining 34 parishes, the figures range between these two extremes. By adding together the figures returned from all the parishes, we find that, in the whole metropolis, during the three years mentioned, there were

132,303 vaccinations of children under one year of age, of which number, 127,858 were successful, or 96.64 per cent. We may, then, I think, fairly take this percentage as our standard for comparison.

Does the use of animal vaccine furnish equally satisfactory results? I regret that I am not able to present a complete report of the results obtained with the animal vaccine which I have furnished in such large quantities to the profession during the last three years, in my capacity of director of the vaccine department of the New York Dispensary. I have, however, through the kindness of a few of our professional brethren in the cities of Boston (and vicinity), Buffalo, and Baltimore, been favored with reports of the results, so far as known, in a very large number of vaccinations done in those cities, with the animal virus, during a period beginning in the fall of 1872, and ending early in the spring of the present year. The gentlemen to whom I refer are, Dr. E. C. W. O'Brien, Health Physician of Buffalo; Dr. Norton Folsom, Resident Physician of the Massachusetts General Hospital, Boston; Drs. R. M. Otis, S. W. Driver, and J. T. G. Nichols, of Cambridge, Mass.; Dr. W. T. Jones, of Baltimore, Maryland State Vaccine Agent; Dr. G. G. Worthington, and a physician of Baltimore, whose name is not given, but who reports through Dr. Jones. Small-pox was prevalent in all of these cities, and the vaccinations referred to were, to a very great extent, done upon persons who had previously been successfully vaccinated. The whole number of vaccinations reported amounts to 78,176. It is to be noted, with regard to the 54,140 cases reported by Dr. O'Brien, that about 3,500 of them were done with humanized virus. The cases were not distinguished in his report, so that it is not known how many of these 3,500 were *primary* and how many *secondary* cases. But, as there were no failures in primary cases, the value of the report is not invalidated.

By an analysis of these reports, I have constructed the following tables:

TABLE I.—PRIMARY VACCINATIONS.

	Whole number.	Inspect-ed.	Success-ful.	Unsuc-cessful.	Success per cent.
First trial:					
Dr. O'Brien..............	4,793	4,322	4,322		100.
Dr. Otis..................	22	22	22		100.
Dr. Driver................	157	157	155	2	98.72
Dr. Nichols...............	16	16	16		100.
First trial with this virus, after other alleged bovine virus had failed once:					
Dr. Otis..................	15	15	14	1	93.33
First trial with this virus, after other alleged bovine virus had failed twice:					
Dr. Otis..................	2	2	2		100.
Second trial with this virus, after other alleged bovine virus had failed once:					
Dr. Otis..................	1	1	1		100.
Total primary vaccinations....	5,006	4,535	4,532	3	99.93

TABLE II.—REVACCINATIONS, *including those of Persons who had had Small-pox, modified or unmodified, natural or inoculated.*

	Whole number.	Insp'ct-ed.	Suc-cessful.	Partially successf'l.	Unsuc-cessful.	Success per cent.
First trial:						
Dr. O'Brien..............	49,347	25,656	23,547		2,109	91.00
Dr. Otis..................	54	54	33	13	8	61.11
Dr. Folsom...............	42	42	34		8	80.90
First trial with this virus, after other alleged bovine virus had failed once:						
Dr. Otis..................	18	18	3	10	5	16.66
Second trial:						
Dr. Otis..................	8	8		2	6	00.
Dr. Folsom...............	6	6	5		1	83.33
Second trial with this virus, after other alleged bovine virus had failed once:						
Dr. Otis..................	4	4		1	3	00.
Third trial:						
Dr. Otis..................	3	3			3	00.
Total secondary vaccinations..	49,482	25,791	23,622	26	2,140	91.59

Table III.—Vaccinations of Persons who had had Small-pox, *modified or unmodified, natural or inoculated.*

	Whole number.	Inspected.	Successful.	Unsuccessful.	Success per cent.
Dr. O'Brien	531	531	264	267	49.71
Dr. Folsom	2	2		2	00.
Anonymous physician, of Baltimore	35	35	16	19	45.71
Totals	568	568	280	288	49.29

Table IV.—Vaccinations, *concerning which it is not stated whether they were primary or sscondary.**

	Whole number.	Inspected.	Successful.	Unsuccessful.	Success per cent.
Dr. Jones	17,738	17,738	13,590	4,148	76.61
Dr. Worthington	1,758	1,758	1,580	178	89.87
Anonymous physician, of Baltimore	2,149	2,149	1,369	780	63.70
Dr. Forster	2,049	383	250	133	65.27
Totals	23,694	22,028	16,789	5,239	76.21

It will be seen, sir, that these reports show a very high percentage of success—decidedly higher than the standard with which we set out. I would particularly call attention to the great proportion of successful revaccinations. With one exception, the gentlemen who have furnished these reports have not classified their successful results as "complete" and "partial." Considering that this is generally done by those who report results of revaccinations, it is to be regretted that it was omitted in these instances; I will, however, take the opportunity of expressing my decided conviction that the common custom is erroneous. In any case, the result is either success or failure, and we may classify degrees of imperfection *ad infinitum*, to no good purpose. But, this is not the main point to which I would call attention: I would lay particular stress upon the fact that these figures are not to be taken as exceptional, but that, in general, a much greater degree of success

* "The subjects are presumed to have been mostly adults and school-children, and it is stated that "a number of them had had small-pox."

ought to be attained in revaccinations than the profession at large are aware of. During the past summer I had the opportunity of bringing these results to the attention of the British Medical Association, and I was then much gratified to hear Dr. Grieve, who was in charge of the Hampstead Hospital during the last epidemic of small-pox in London, express the same view, he taking my figures as a corroboration of the opinion.

These results, then, such as I have read them to you from these tables—results obtained, be it noted, with stored lymph sent by mail or express to distances of from two hundred to more than four hundred miles, often a week old before it started, the vaccinations extending over a period of several months (the results being therefore not a mere "lucky hit") —demonstrate, it seems to me, the utter groundlessness of the opinion, which even now frequently finds expression, that vaccination with animal virus is unsatisfactory in regard to the degree of success obtained.

When I began my investigations, this opinion was almost universal in this country, so that I found it a matter of great difficulty to persuade even my personal friends in the profession to give the Beaugency virus a trial. The wide-spread prevalence of this prejudice would seem to be due, in so far, as least, as recent events are concerned, very largely to the unfavorable report of Dr. Seaton (in the "Twelfth Report of the Medical Officer of the Privy Council"), who visited various Continental cities for the purpose of observing and reporting upon the practice of animal vaccination as there pursued. It must be admitted, sir, that, considering the facts which came under his observation, or were brought out in his subsequent correspondence with those having charge of the animal vaccine service in certain cities, he could not have made any other sort of report than that which he did make. The simple truth is, that the results which those gentlemen obtained were far from creditable, and they have, no doubt, greatly hindered the progress of animal vaccination; all owing to what I conceive to have been radical errors in the methods adopted, but which it does not come within my present purpose to discuss.

Proposition II.—*The vaccinia produced by the use of animal vaccine is genuine, and as thoroughly protective against small-pox as is that obtained by the use of any other form of vaccine.*

This proposition is supported, *a priori*, by the more characteristic development, in the generality of cases, of the pock produced than of that which follows the use of long-humanized stocks. This has been often enough described, so that I need not dwell upon it now. I will remark, however, that opportunities of verifying the differences which have been noticed are rapidly growing fewer, owing to the fact that every year there is less and less long-humanized lymph used.

I have here Depaul's *brochure* entitled "Sur la Vaccination Animale, Discours prononcé à l'Académie Impériale de Médecine dans la Séance du 3 Septembre, 1867." On page 46, I find a passage which I will translate, as follows: "In 1865 I myself attempted in vain to revaccinate three children who had already been vaccinated with cow-pox lymph, one month, six weeks, and two months, previously. In one case I went still further: upon a child vaccinated more than six weeks before, I inoculated, without success, *variolous virus*, taken a half-hour previously from a patient in the fifth day of the eruption. I have several times seen children, vaccinated only a few days' before with cow-pox, living with impunity in the same room with one or more small-pox patients."

In my own experience there have occurred not a few instances of undoubted exposure to small-pox, in which, after revaccination with the animal virus, the disease has not shown itself. Possibly it would not have done so even if the revaccinations had not been done, but the accumulation of such cases must be allowed very great weight.

It is not to be supposed that small-pox can never occur after vaccination with animal virus, even within a comparatively short time. I am, however, aware of only one instance in which it has occurred in this city. This case happened in the practice of the Northern Dispensary. A child was vaccinated there, by Dr. Warner, the house-physician, in December, 1871, the

animal virus being employed. Dr. Warner inspected the case on the eighth day, and considered the pock to be pursuing a regular course. I think there was no further examination, so that, for all that we know to the contrary, the febrile reaction and the areola may have been wanting. At any rate, the child was attacked, during the following March, with a very mild varioloid—so mild that Dr. S. F. Morris, who saw the child on behalf of the Board of Health, felt some hesitation, at first, as to the diagnosis. The child was sent to the hospital, where it made a rapid and complete recovery.

Such cases, though happily not common, are met with occasionally, and were in former years, long before animal vaccine was used. In this book (the third volume of Barthez et Rilliet's "Traite clinique et pratique des Maladies des Enfants," second edition, Paris, 1861), I find, on page 117, the statement that "the youngest children are not wholly proof against post-vaccinal small-pox." "Thus," these authors go on to say, in a foot-note on the same page, "of 96 children sick with small-pox at the *Hôpital des Enfants*, thirty had been vaccinated. Their ages were as follows: two were two years old; five were three years old; one was four years old; three were five years old; two were six years old; one was seven years old; two were eight years old; four were nine years old; two were ten years old; four were eleven years old; one was twelve years old; three were fourteen years old."

A Fellow of this Academy (Dr. Leaming), who, as I see, has done me the honor to be present this evening, and who, in former years, was one of the house and vaccine physicians of the Northern Dispensary, will acquaint you, I have no doubt, as he has already acquainted me, with the facts in connection with certain cases of post-vaccinal small-pox in infants, which came under his observation at that time—long before the introduction of animal vaccination into this country.

I could name a physician, also Fellow of the Academy, who, although now only thirty years of age, has had small-pox twice. His father, himself a physician, vaccinated him successfully at the age of thirteen months. At the age of thir-

teen years, he had an attack of modified small-pox. He was afterward repeatedly, but unsuccessfully, vaccinated with good current lymph; yet, at the age of twenty-two years, he suffered from a second attack of small-pox, and this last time the disease appeared in its *unmodified* form. All his vaccinations were done with humanized lymph.

I mention these facts, not as novelties, for they are not so to those who are familiar with the history of small-pox and vaccination, but simply to illustrate the position I take, viz., that small-pox, which often occurs after vaccination, and occasionally after previous small-pox, is no more likely to occur after the use of animal than after that of humanized vaccine, but that neither the one nor the other is an absolute preventive of the disease.

Proposition III.—*The use of animal vaccine does not entail the dangerously severe local inflammatory and general febrile complications which have been attributed to it by some writers.*

In support of this proposition I have, as a matter of course, only negative evidence to offer. I may say, first of all, that, in our experience at the New York Dispensary, there has not yet been a single case of such complications, not even so much as a suppurating axillary gland, during the three years that we have used the animal virus; whereas, during the previous four years that I was in charge of the vaccine department, when we were using humanized virus, such complications were not very uncommon. During that time I even observed one case of chronic pyæmia following vaccination. It is true that, in adults, the animal virus occasionally causes a smart febrile reaction, with nausea, chilliness, pain in the back, etc., lasting for perhaps twenty-four hours, but never giving occasion for any anxiety; and that *eruptive vaccinia*, if we may admit the term, is rather more frequently observed than after the use of the old stock. But this is rare at best, and is, moreover, utterly free from danger. I think that the general experience of those who have used this virus will bear me out in this statement, although it is not to be assumed that never,

under any circumstances whatever, will dangerous complications follow the use of this stock. Whenever these occur, however, our experience justifies us in assuming them to be due to extrinsic causes, as turned out to be true in regard to some cases reported to me by Dr. Alfred North, of Waterbury, Conn., last spring. Dr. North, who was using this virus, reported several cases of severe inflammatory (erysipeloid) complications, but he also stated that his fellow-practitioners in Waterbury were getting the same results, although in all probability they were using different stocks. At all events, I had not furnished them with their material. This fact, together with the absence of any such results in the practice of other gentlemen who had been supplied from the same lot with Dr. North, led us to conclude that the complications alluded to were owing to some contagion localized in Waterbury at the time, and wholly independent of the character of the vaccine.

Proposition IV.—*Animal vaccine is, equally with humanized vaccine, capable of preservation for a considerable length of time, for transmission to remote parts.*

The reports which I have received in regard to virus sent to great distances are not as numerous as I should like them to be. What there are, are, with very few exceptions, favorable, so far as regards quill-slips and tubes, particularly the former. The only instance, so far as I am aware, of the deterioration of quill-slips *in transitu*, when sent off in any considerable quantity, occurred in the terribly hot summer of 1872, in the case of several lots sent to San Francisco. I have sent them to all parts of the United States, including the Gulf States, at every season of the year, and the results, so far as I have been made acquainted with them, have been almost invariably satisfactory. I may mention a striking instance which occurred in my own experience during last summer. Being in London, I wrote to Dr. W. F. Cushman, who kindly took charge of the service during my absence, asking him to send me some quill-slips with which to vaccinate my own child. He sent me, by mail, eight quill-slips, which he

had charged on the 9th of September. The letter reached London after I had left there, and was forwarded to me at Liverpool, arriving there the day before I sailed for home. Without opening the package, I placed it in my state-room trunk, and on arriving at New York, October 4th, I took out the package, intending to test the vaccine on a calf at the first opportunity. It escaped my attention, however, and was not used until October 7th, lying neglected in a warm room in the mean time. On the date last mentioned, I used it for making six insertions on a calf, and four of these were productive of perfect pocks.

This I do not look upon as any thing extraordinary, for I have long felt convinced that, within reasonable limits, the length of time during which vaccine virus has been stored is one of the least important elements to be taken into consideration in accounting for the success or failure of any given series of vaccinations. I am aware that this is contrary to the general opinion, and I presume that a majority of the physicians of New York are unwilling to use lymph which has been kept more than forty-eight hours, but I can only characterize this feeling as a prejudice—a prejudice not altogether devoid of evil effects; for it often happens that lymph of one particular lot is more energetic, even after the lapse of a considerable length of time, than that of another lot which may be much more recent, so that, in any case of positive exposure to small-pox, I prefer to rely on lymph which I have already tested to my satisfaction, rather than on that which is freshest.

In regard to tubes, I have received very few reports, but I have sent them to Panama and to Japan, and have been informed that they proved satisfactory.

Crusts form an apparent exception to my proposition. The bovine crust, as at present cultivated, does not retain its virtues so long as humanized crusts do. The reasons for this, however, attach rather to what happens to it before it is shed, than to any special difficulty in its subsequent preservation. Crusts of all sorts I consider to be, beyond all doubt, the very worst form of vaccine, so that this is a matter of comparatively little importance.

PROPOSITION V.—*The use of animal vaccine eliminates one of the elements in the causation of vaccinal syphilis, and thus helps to silence the anti-vaccination agitators.*

The essential portion of this proposition needs, I will venture to say, no argument. The corollary which it includes was powerfully illustrated by last winter's experience in Buffalo. When Dr. O'Brien obtained his first supplies of animal virus from me, he had, as he informed me, been fighting the epidemic for the better part of a year. His course of action was seriously hampered by the opposition which he met with in vaccinating the people. This opposition practically vanished on his being able to announce that he was employing animal virus, so that he was enabled to perform the large number of additional vaccinations mentioned in his report, and, in a few weeks, put a stop to the disease.

PROPOSITION VI.—*Animal vaccination is the most trustworthy safeguard against failure of the supply of vaccine in emergencies.*

To illustrate the truth of this statement, Mr. President, I will simply relate, before I close, that, one day last winter, I received from the Board of Commissioners of Public Schools, of Baltimore, a telegraphic order for quill-slips sufficient for 6,000 vaccinations. I sent the whole amount before the expiration of twenty-four hours. Now, I do not think that, under ordinary circumstances, this quantity of lymph could be obtained from children's arms, in so short a space of time, without violating the rules which should be observed in regard to the choice of subjects, to say nothing of other important details connected with the matter of taking lymph.

DR. ELLSWORTH ELIOT stated that he had used the animal virus almost exclusively during the past two years, and could bear testimony to the excellent results which it produced. In former years he had employed humanized virus, obtained sometimes from his own patients, and at other times from the dispensaries. So far as he was concerned, he had considered that he was using a good enough material, but he had often

had to delay the vaccination of children, on account of the dislike which parents frequently expressed to having their children vaccinated with lymph from dispensary patients. They would ask him to wait until he could get lymph from his own cases. Since he had begun to use the animal virus, however, he had met with no difficulty of this sort. He had been very much gratified, too, at the degree of success which it had afforded, it never having failed, in his hands, in a single case of primary vaccination thus far. The results were benign and in every respect satisfactory. Indeed, it was difficult for him to account for the determined opposition which a few of his professional friends made to the use of the bovine virus. He instanced one of these, who, on a certain occasion, gave expression to a decided feeling of that sort. He (Dr. Eliot) proposed to this gentleman to visit with him two infants whom he had vaccinated with the bovine virus that day week. On inspecting the pocks, the gentleman was so struck with their fine appearance, that he requested some of the lymph from them for his own use. Dr. Eliot was under the impression, however, that his friend was not yet thoroughly converted.

Dr. Eliot expressed himself as having listened with much interest to Dr. Foster's modest story of the results which had grown out of his efforts, and would remind the Academy that Dr. Foster had previously, in an essay which received the prize, for the year 1872, of the Alumni Association of the College of Physicians and Surgeons, done good service, in a literary way, in connection with this subject. There were two names, among those of New York physicians past and present, of gentlemen to whom, in Dr. Eliot's opinion, the profession in New York should feel deeply indebted in connection with the subject of vaccination. The first was that of Dr. Valentine Seaman, who first introduced the practice of vaccination into this city; the other was that of Dr. Foster, who had furnished them with their strongest weapon for overcoming popular prejudice against the practice. Highly as he appreciated Dr. Foster's efforts, however, he felt called upon

to confess himself as one of those who, as Dr. Foster expressed it, entertained the prejudice in favor of very fresh lymph. He was always careful to procure his supplies as soon as possible after it had been brought down from Greenwich, and to this fact he attributed, in no small degree, his successful results.

Dr. O'Sullivan stated that his experience, a very large one, more particularly in the vaccination of school-children, had been, for the most part, with the humanized virus, as issued for so many years by Dr. Loines, of the Eastern Dispensary, in whose practice in that institution, as well as at Quarantine, were vaccinated upward of 300,000 with satisfactory results. In his own experience of school-children, in the house-to-house vaccinations by the Board of Health, and in the hands of family practitioners, this virus had always proved thoroughly trustworthy. He saw no reason to doubt that the animal virus was equally so, only he had been under the impression that the Board of Health, which had experimented with it to a certain extent, had come to the conclusion that it was not, in certain cases, quite as satisfactory as the results from humanized virus. He had no doubt that it was carefully prepared, and was now informed that the animal virus used was not furnished by Dr. Foster, but was prepared under the supervision of the officers of the Board of Health.

Dr. O'Sullivan thought that the importance of this subject was deserving of something more than an evening's discussion, and, while not prepared to make any motion or request to that effect, he would suggest that the Committee on Vaccination might appropriately take the matter under consideration. He thought it due to the Academy that that committee should give more attention to this subject.

Dr. Caro had always, until recently, used the humanized virus. He had never seen any evil effects which he could attribute to it, and did not believe that, if proper precautions were taken (such, for instance, as not to take blood together with the lymph), there was ever any danger of conveying any constitutional taint. Recently, he had vaccinated five chil-

dren with animal virus procured from Dr. Foster, and in three of the cases the result had been successful. The two others had not yet been inspected. In one of the cases he had used this virus for the reason that he had, some time ago, vaccinated another child in the same family, and on that child, when the scabs fell off, the pustules were found ulcerated, enlarging in a short time to a considerable size, giving a great deal of trouble before healing. In this second child, on the sixth day after the vaccination, he had been called to see the case, and found a vesicular rash broken out over the child's body. The mother accused him of having used small-pox virus, but he was, to his great relief, able to clear himself at once in her estimation, by assuring her that he had used lymph direct from the calf, without going through the tedious and perhaps futile process of argumentation which would have been necessary had the virus been taken from the human subject. He was of opinion that some dormant disease, such as tuberculosis, etc., could be kindled in the system through the crisis produced by inoculation.

Dr. Caro coincided in the belief that one vaccination was sufficient for the lifetime of any individual, but nevertheless he never objected to revaccinate those who desired it. He always vaccinated on both arms, making two inoculations on each arm (when permitted), but never less than one.

Dr. Leaming's experience with the animal virus furnished by Dr. Foster had been quite recent and not very extensive, but he could state that the results had been satisfactory, so far as they went. Dr. Foster had alluded to his (Dr. Leaming's) experience in the matter of small-pox occurring within a short time after vaccination. He would mention the following case: Some years since, when he was vaccine physician of the Northern Dispensary, he vaccinated a child, with the same lymph with which he was vaccinating others—the lymph current in the dispensary—from whose arm, which presented the typical appearances, he took lymph on the eighth day. Having used up the lymph thus obtained, and his supply from other sources being scanty, he revisited this child at its home,

for the purpose of obtaining the crust, but was very much surprised to find it exhibiting the eruption of varioloid.

Dr. Leaming believed not only in the necessity of revaccination, but in repeated vaccinations—repeated at intervals of perhaps a year, until it could no longer be made to produce a local result. One vaccination might protect a certain proportion of subjects, but he considered it demonstrated that, in some persons—perhaps 25 per cent.—not even temporary protection, of a perfect character, would result from a single vaccination. He had felt this so strongly, that he had laid the matter before the Board of Trustees of the Northern Dispensary, during his incumbency of the vaccine physicianship, and measures had been taken by them to impress upon the community the necessity of repeated vaccination.

Dr. Leaming here read the following extract from his report, as vaccine physician, to the Trustees of the Northern Dispensary (*see* "Twenty-fourth Annual Report of the Trustees of the Northern Dispensary, for the Year ending April 4, 1851," p. 34).

"In 1842, the Academy of Sciences in Paris offered a prize for the best treatise on 'The Value of Vaccination and Revaccination.' Thirty-five candidates responded to the call. M. Serres made a report on the comparative merit of the essays, in behalf of the committee appointed to decide, which was adopted by the Academy. He says: 'In Wurtemberg, 42,000 persons who have been revaccinated have only presented eight cases of varioloid; whereas one-third of the cases of variola have latterly occurred on persons who had been vaccinated.'

"The following are the 'results of revaccination in the Prussian army, during 1849 : . . . There were 51,637 individuals revaccinated, of whom 39,116 had distinct cicatrices of the former vaccination, 8,706 had these in an imperfect condition, and 3,815 were destitute of them. The vaccination pursued a regular course in 30,457, an irregular one in 8,467, and failed in 12,713, succeeded on repetition in 2,862.

"'Thus, then, of the 51,637 vaccinations, 33,319 were quite

successful; vesicles running a normal course being produced. This proportion amounts to 64 per cent., and is about one per cent. more than was obtained in 1847 and 1848.'"

Dr. Janes had, in former years, as a matter of course, used the humanized virus solely, and had felt no reason to be dissatisfied with it. Ever since his connection with the Board of Health, his experience had been, in a great majority of cases, with the same virus.

When the Board of Health first began its system of public vaccination by house-to-house visitation, the gentlemen of the vaccinating corps were each given a clean lancet, and instructed to note down and report carefully any irregularities which might occur in their results. This they did faithfully, and, out of some 30,000 vaccinations done during the month of June, 1869, only the cases of sufficient irregularity to call for any special investigation were reported.

Very soon after the organization of the vaccinating corps, he had learned something which he would not have thought probable before, namely, that there were very few good vaccinators. It was no uncommon thing to find that, of two gentlemen vaccinating with the same virus, the one would obtain as great a degree of success as was to be expected, while the other would fail in very many of his cases. It was found useless to try to overcome this unskillfulness by didactic instruction, but it was speedily corrected—generally in the course of half a day's work—by sending out the unskilled vaccinator in company with one whose skill was known. It was quite evident to him, therefore, that the skill of the vaccinator was one of the chief elements in determining the results.

He regarded errors in collecting virus as another source of failure. It was a delicate operation to so open a vesicle as to obtain from it nothing but the pure lymph which it contained. This should be done before the areola is established, otherwise we should have the products of ordinary inflammation, which would vitiate the purity and efficacy of the lymph. He had noticed that some public vaccinators, in collecting lymph,

were ambitious to obtain a large supply from each vesicle; and that by continuing to irritate the vesicle, even after it was exhausted of its lymph, a flow of serum was obtained which appeared very well on the quill, but contained but little if any genuine lymph. He would, therefore, insist that in public vaccination the duty of collecting lymph be intrusted to persons especially skilled in this branch of the service. He did not wish these criticisms to be understood as applying to the family practitioner, but to those young men who see their first active service as public vaccinators, without previous training and experience.

He thought it not quite fair to count the experience of the Board of Health against animal vaccination as conducted by Dr. Foster, for the stock of bovine virus which they had used was entirely different from Dr. Foster's, and he had never been able to satisfy himself of its origin.

In regard to Dr. Caro's case, the eruption might have been vaccinal; but, in some of the cases of so-called eruptive vaccinia that had come under his observation, the eruption existed only on those parts of the body which could be reached by the patient's hand (of the unvaccinated side), so that he was disposed to ascribe the formation of such pocks to the transfer of lymph from the original pock, by the patient's scratching it, and then scratching other parts of the body.

Dr. Harris regretted that, owing to the lateness of his arrival, he had not heard the whole of Dr. Foster's remarks. He had, however, felt a great interest in his investigations, and had been quite prepared to hear of the attainment of such results as had been related. He considered that vaccination in the city of New York was very efficiently done, as evidenced by the comparative immunity of the city during the general small-pox prevalence of the past three years. Although the old stock of humanized virus had been used in perhaps the majority of cases, even during these three years, yet he was quite willing to recognize the fact that the popular opposition to vaccination had been very notably diminished by the well-directed and patient endeavor of Dr. Foster to

furnish the profession with a trustworthy supply of animal vaccine. The fact was now acknowledged that this method of meeting the ordinary requirements for fresh and efficient virus was completely satisfactory, and that, for the great and sudden demand that occurs wherever small-pox begins to prevail, this method of supplying good vaccine was unspeakably important.

The family physicians had justly retained their allegiance to the doctrines of Jenner, and these experienced practitioners were faithful vaccinators, scrupulous in the selection of the vaccine they employed, and careful in their vaccinations. Such physicians were not slow to recognize such service as Dr. Foster was now giving.

In closing the discussion, Dr. Foster remarked that several collateral points had been raised, which, while not coming strictly within the scope of his propositions, were of great interest, but want of time would forbid their further discussion at the present time.

In regard to Dr. Caro's case, the eruption might have been (1) chicken-pox, which sometimes very closely resembles varioloid; or (2) small-pox, the exposure and consequent infection having taken place several days before the vaccination; or (3) the so-called eruptive vaccinia, although the sixth day was rather early for it to appear. In regard to eruptive vaccinia, he was already on record as entertaining the view, which Dr. Janes had mentioned, that it was, in the great majority of cases, not truly eruptive, but occasioned by the accidental transfer of lymph from the original pock. If this were true, the fact of its occurring somewhat oftener after vaccination with bovine than with humanized lymph would form an additional evidence of the activity of the stock.

Dr. Caro stated that he did not wish to be understood as imputing the eruption, in the case which he had referred to, to the fact of his having used the animal virus, neither did he consider the eruption as of any great consequence.

STATED MEETING, DECEMBER 18, 1873. DR. AUSTIN FLINT, PRESIDENT, IN THE CHAIR.

THE PRESIDENT announced the death of Drs. J. P. Loines, December 15th, W. C. Roberts, December 9th, Alfred Underhill, December 7th, and Peter Van Buren, December 5th, Resident Fellows of the Academy.

Dr. ADAMS, Corresponding Secretary, announced the death, December 14th, of Prof. Louis J. R. Agassiz, a Corresponding Fellow of the Academy.

Resolutions in memory of Drs. Roberts and Underhill were read and adopted. Dr. S. O. Vanderpoel, by invitation, read a paper entitled "General Principles affecting the Organization of Quarantine, with a Brief History of the Institution."

The Academy then adjourned to January 8, 1874.

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* Subsequent to the printing of the table on page 328, giving the mortality of various cities, official returns were received from several Italian cities, giving different (*verified*) figures from those previously transmitted. According to these, the death-rate of Rome was 40.6 in each 1,000 inhabitants—of Turin, 30.4—of Milan, 34.6—of Florence, 35.6—of Bologna, 34.5.

Returns were also received from a few other cities, whose death-rates were as follows: Trieste, 46.0—Madrid, 52.7—Cairo, Egypt, 81.8—Alexandria, Egypt, 63.4. C. P. R.